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AND

JOURNAL OF THE USEFUL ARTS.

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THE

MAGAZINE OF POPULAR SCIENCE,

AND

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A POPULAR COURSE OF GEOLOGY.

INTRODUCTION.

SIR JOHN HERSCHEL has said that Geology, in the magnitude and sublimity of the objects of which it treats, ranks in the scale of the sciences, next to Astronomy; to which we may add, that it will ever be more generally cultivated, because a knowledge of it is more easily attainable. It may be successfully pursued without that severe preparatory discipline of mathematical study which is required of the votaries of astronomy, before they can advance even to the threshold of her temple. In making this assertion, we by no means deny the dependence of geology on the other sciences; we admit, on the contrary, that he who would be a perfectly accomplished geologist, ought to be familiar with the whole circle of them. He ought to be thoroughly versed in mathematics and general physics, in order that he may know what are, and what are not, sound data on which to found his inferences—he ought to be skilled in mineralogy, that he may know the proximate constituents of rocks. Of the general results of chemistry he must not be ignorant, and he will find it a great advantage to be expert in chemical analysis. The organic remains entombed in the strata, will make constant demands upon him for a knowledge of zoology in all its branches, and in particular he ought to possess such an intimate acquaintance with those nice distinctions which constitute specific differences in conchology, as of itself requires the study of a whole life, and such a profound knowledge of comparative osteology, as enabled Cuvier, from the examination of detached bones, to remodel the entire skeletons of animals of unknown genera: such is the harmony of proportion, the adaptation of means to ends, and of parts to uses, which the wisdom of the Creator has manifested in the structure of organic bodies. The geologist ought moreover to be a botanist of the highest order, and in the most extensive sense of the term. He ought to be able not merely to refer a plant to its place in some artificial system, by counting its stamina,—a process which he will rarely, if ever, have an opportunity of applying to the fossil vegetation of former worlds,—he ought to be able, from the examination of a stem, a leaf, or a seed-vessel, to determine the natural group to which the plant belongs, and by pointing out its habits, to throw light on the circumstances under which the stratum containing it was deposited. He ought, moreover, to be a good draughtsman, and a skilful practical surveyor. Acquirements so varied and extensive as these are attainable by few, and yet much may be done in geology with a *very limited proficiency* in these branches of knowledge. *Without a very profound acquaintance* with any of them we may master all the facts of the science, and all the inferences deducible from them, and what is more, we may be qualified to institute active original research, and to enroll our names on the list of those who have added, by their discoveries, to the sum of human knowledge,—for geology is a science of observation. It is, moreover, a young and advancing science, many of whose data remain to be collected, and in the collection of them there are few who cannot assist. When we know about a dozen of the most common, simple minerals,—when we can recognise their combinations in rocks,—when we know the technical terms of the science, and can distinguish crystalline from sedimentary, stratified from unstratified rocks,—and when we know the order in which the strata composing the earth's crust succeed each other, we are qualified to examine nature for ourselves, and to study geology, where it is best studied, in the field. We require not the expensive observatory or laboratory of the astronomer or the chemist, —all we want is a good hammer, and a strong arm to use it, active legs, a quick eye, and sufficient common sense to enable us to reason upon what we see. For the rest we may trust to the assistance of our fellow-labourers, and to that community of feeling by which they are ever animated; for geology is eminently a social science, and the great and rapid advances which it has made within the last few years, are in a great measure to be attributed to that division of labour, and that mutual co-operation which can only be effected by numbers acting in concert. "These volumes," says Mr. Murchison, speaking of the *Transactions of the Geological Society of London*, "must ever be valuable as the true records of our scientific progress; but great as may have been the acquirements of their authors, few indeed are the memoirs which have been completed without the aid of other distinguished fellows of the society, who, each in the branch of natural knowledge for which he stands pre-eminent, comes to the assistance of his wandering associate, and enables him to clothe his memoir in an appropriate dress. For where is the working geologist who, unassisted, can unravel the delicate and obscure complications of fossil organic structure? Do his fossil shells require to be identified, has he not the assistance of a Sowerby? and if these types of a former state of nature call for a comparison with existing species, is not a Broderip ever prompt in affording him the result of experienced discernment, and in unfolding the riches of his unrivalled cabinets? If he meet with difficulties in the determination of Mammalia, are not a Mantell and a Clifft at hand, to explain their relations and define their characters? Or if bewildered in the obscurity of fossil vegetation, is he not assisted by a Lindley? Have not, in fine, a Turner, a Prout, a Faraday, and a Herschel been willing instruments in enabling him to explain those laws of chemical change, without which the recondite parts of the science might have remained in utter darkness? Surely every contributor to our Transactions will acknowledge with gratitude the aid he may have received from several of our most gifted members, who, unambitious of personal fame, have been contented with the delightful consciousness of being sure, though silent, instruments in urging on the advance of truth. It is this kindly principle of co-operation, this true latent heat of the Geological Society, so ready to manifest itself on every *occasion fitted to call it* forth, which, warming and vivifying our endeavours, gives to our proceedings their consistency and their strength, and enabling us to grapple with our hundred-headed science, constitutes the mainspring of our prosperity*."

If geology yields to astronony in the sublimity of the objects of which it treats, and in being unable to bring its truths within the pale of mathematical demonstration, it possesses this advantage, as a science for popular study,—that it presents them to us in a more palpable shape. One of the first, and hardest lessons which we have to learn of the astronomer, is to discard impressions founded on what we deem the evidence of our senses. Misled by these, we have been accustomed to consider the earth as at rest, and the sun as making a daily circuit round it. The astronomer demonstrates the contrary, and corrects our notions as to the relative size of the celestial bodies. He determines the figure of the earth, and the form of the orbit in which it moves, and ascertains the velocity of its daily and annual motion. He determines the magnitude of the sun, and its distance from us, and shows us other planets revolving round it, with their attendant satellites, and obeying the same laws which regulate the motions of that which we inhabit. He pushes his discoveries to the utmost verge of the visible creation; and, by the aid of powerful optical instruments, resolves those twinkling points, which we call the fixed stars, into groups of suns moving round each other in "mystic dance;" and he demonstrates that those laws of gravitation, which regulate the fall of bodies on the earth, are universal laws, which are obeyed in the remotest system of worlds within the reach of mortal ken. He can calculate, not only the motions of the bodies composing the planetary system, and point out their relative positions for any period of past or future time, but he can predict, within a few hours, the return of those mysterious wanderers, whose course extends into the regions of space, far beyond the remotest planet of our system, and whose periodic times are measured, not by days, but by years. It may be well said that in all this there is an overwhelming sublimity. The distances treated of are so immense, and the time required to complete some of the celestial cycles so vast, that they elude the grasp of our comprehension;—we may talk about billions and trillions of miles, and myriads of years, but we have scarcely a less vague conception of them, than we have of infinity of space, and eternity of time. Geology, on the contrary, appeals directly to our senses. She lays open the ground on which we tread, and convinces us, that the vast secular periods of the astronomer, which, with him, are mere abstract arithmetical truths, which he cannot prove to have had an actual existence, may all have been required for the production of those changes on the surface of our globe of which we witness the monuments. She proves that our present continents, with the most elevated of their mountains, were formed at the bottom of the ocean; and that our hardest rocks were once sand, and gravel, and mud derived from the wearing down of land no longer in existence. She points to a bed of rock, a few feet in thickness, teeming with the remains of organic life; and, from the successive generations of individuals which it contains, and from other indications, into which we will not at present enter, proves that a very long period must have been required for its formation. She then conducts us through a vast series of similar sub-marine deposits five or six miles in depth (how exposed to our view will be explained hereafter) abounding with the remains of plants and animals, and containing, not only the relics of successive generations of individuals, but of successively created races, each group of strata having its peculiar group of organic remains. She unfolds, page after page of this great book, this wondrous record of the changes our earth has undergone, and of the tribes of beings by which it has been peopled during a series of periods, of long but unknown duration, before it was inhabited by man. She first leads us to those formations now in progress in which he and his works are imbedded, together with the remains of this contemporary species of plants and animals. This page of the world's history is soon read.

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* *Murchison's Anniversary Address—Proceedings of the Geological Society.*

Man is found to be but a creature of yesterday, compared with the globe he inhabits, and with the other beings with which it has been peopled. The very species of plants and animals now existing are found to have been called into being before him, for their remains occur in older, that is in deeper strata. The remains of existing species are found to be gradually intermixed with those of species that have vanished from the face of the earth. The proportion of extinct species increases as we descend. We come to lower beds still, in which not only extinct species occur, but extinct genera. The forms of organic life recede more and more from existing types, and they diminish in number in the lower rocks, till, at last, we lose all traces of them entirely. During our progress through this vast series of rocks, evidently of submarine formation, we meet with others bursting through them, which are as clearly of igneous origin, and derived from below. The lowest rocks we meet with are of this igneous character, and contain no organic remains. It may be that we have reached the records of a period when the world was unfit for the support of animal and vegetable life. It may be that the earlier pages of its history have been torn out, and that the rocks in question once contained the remains of still older races, but that all traces of them have been obliterated, by the fusion to which the rocks have been exposed. At all events, we have reached the dark ages of the earth's history, and we close the book. With speculations on the creation of the world,—on the mode in which it was reduced from a chaotic state,—and of the causes which gave it its present figure, we have nothing to do. They were favourite themes with the geologists of former days, whose wild reveries threw a discredit on the science, from which it was long in recovering, even after its votaries began to walk in a more sober path. The cosmogonists, as they are called, applied themselves to the invention of modes in which worlds might be created, with an industry, which, if applied to the observation of phænomena in the world around them, might have led to important knowledge. But that was too humble a task for them. They preferred the field of imagination, and all their labours in that field tended but to bring chaos back again. In contemplating their worlds, it would seem as if "nature's journeymen had made them," and had "not made them well." And yet they arrogated to themselves a command over all the powers of nature, and they were not *sparing in their use of* them. They made the globe solid or hollow,—

they filled it with water or matter in a state of fusion, as it suited their purpose,—they changed its axis of rotation at pleasure,—and when they were in difficulties they called in the aid of a comet, sometimes to produce a deluge, sometimes a conflagration.

But the day for this mode of philosophising passed away, and geology, for the last half century, has been a science of observation and induction, not of invention. Geologists now wisely limit themselves to an investigation of the *crust* of the earth; and vast as a series of deposits five or six miles in thickness may appear to us, the proportion it bears to the diameter of the earth is only as 1 to 1600. The paper covering a globe sixteen inches in diameter, is nearly thick enough to represent it. The series of rocks containing organic remains is found to rest upon rocks which are destitute of them, and which bear evident marks of having been in a state of fusion. The lowest rocks we meet with are of this igneous character. We know that lower still subterranean fires are in full activity, giving rise to volcanoes and earthquakes, and, from the great areas over which the shocks of the latter are simultaneously felt, it is probable that the source of internal fire is deeply seated. The mean density of the earth has been ascertained to be twice as great as that of the rocks at the surface; and supposing the interior to be composed of materials equally compressible with those of which the crust consists, the pressure to which the central parts are subject would cause the mean density to be much higher than it is found to be. A large portion of the interior must, therefore, be occupied by cavities,—or must consist of less compressible materials than the rocks of the surface,—or there must be some expansive force within, capable of counteracting the effect of pressure. Central fire may be this counteracting force. This is all we know of the interior of the earth, and perhaps all we ever shall know.

Astronomers have declared, that they can discover, in the planetary system, no evidence of a period when it differed materially from its present state. They do not deny that it had a beginning, but they tell us that they can discover no traces of that beginning, neither can they see any prospect of an end. He who made it can doubtless destroy it, but its destruction will arise from causes unknown to us. It contains not within itself, as far as our observations extend, the seeds of its own dissolution; for it has been demonstrated that all the perturbations which can be produced in the orbits of the planets by their mutual attractions are periodical, and range within certain limits, slowly increasing during a long lapse of ages; then, as slowly diminishing, and never deviating widely from a mean state. The machine, therefore, having been created and set in motion, and having been so constructed as to be capable of continuing its action through all eternity, may have been left to itself, without the further active interference of its Divine Author. The researches of geology lead to different conclusions. We find that our planet has been subject to great and repeated changes; that there have been changes in the condition of the earth, accompanied by corresponding changes in organic bodies, adapting them to those altered conditions. It is true, that in his investigation of the crust of the globe, the *geologist is as little able* as the astronomer to perceive evidence of *the beginning of things.* It is even doubtful whether he is able to carry back his researches to the commencement of organic life; though the paucity of remains in the earlier fossiliferous strata appears to favour the conclusion, that the still older stratified rocks, which contain no organic remains, were deposited when the ocean was destitute of living beings. It is, however, certain that we repeatedly see the commencement of new races, and are obliged, again and again, to have recourse to a supreme Intelligence and a creative Power. If we examine the marine remains of the strata, we find that whole genera of shells, which in the present seas are most abundant in species, were not in existence till after the chalk was deposited. Other genera again originated about the middle of the series, and soon became extinct, being represented by no species in the tertiary strata, that is, the strata above the chalk. These new creations supplied the place of other races which perished; for some genera are peculiar to the lower groups of rocks, not a single species of them occurring higher in the series than the coal measures. There are a few, and but a few, genera which, commencing in the lowest fossiliferous strata, have endured through all the changes to which the earth has been subject, and have species existing in the present seas. The changes which occurred in the organization of fishes appear to have been greater and more rapid, and exhibit a wider difference between those found above and below the chalk than is observable in the case of molluscs. For fuller particulars on this head we refer our readers to the paper "On recent Researches in Geology," in our second number.

The same proofs of organic changes are afforded by the study of fossil botany. The formations containing vegetable remains may be arranged, according to Professor Henslow, in four groups, representing epochs, during any one of which no very marked difference is observable in the general character of the vegetation; but between any two of these groups the change is striking and decided, most of the genera being different, and none of the species alike. The character of the fossil vegetation of the earlier epochs is also such as to warrant the conclusion, that the plants of that epoch grew under a climate both hotter and moister than that of any part of the earth at present; and, since even in Arctic regions, the fossil plants are analogous to those now growing under the tropics, it seems probable that light as well as heat were formerly more equably diffused.

We can scarcely be said, at present, to have sufficient data for determining what were the animals inhabiting the land while the earlier strata was being deposited at the bottom of the sea. It was at one time supposed that a gradual developement of organic life might be traced through the series of formations from inferior to higher tribes, from beings the most simple to those of the most complex structure. It was thought, for instance, that the lower strata contained only the remains of animals without a vertebral column, such as molluscs and crustaceans. Of the former, the oyster and the whelk may be adduced as familiar examples; of the latter, the lobster and the shrimp. It was supposed that marine vertebrated animals did not exist till after the coal-formation was deposited; nor oviparous reptiles, such as lizards, and turtles till a somewhat later epoch; and that warm-blooded animals and birds were not created till after the chalk was deposited. But these views have been greatly modified by recent discoveries. The inference as to the non-existence of fishes, resting, as it did, upon negative evidence has been entirely subverted. It is now ascertained that their remains occur in strata of all ages, each group of strata being characterized, according to M. Agassiz, by peculiarities in the structure of its fishes; and though the remains of reptiles have not yet been found so low as the coal formation they have been traced down to the magnesian limestone, immediately above it; which gives to them a higher antiquity than till lately they were supposed to possess. Again, with respect to the remains of warm-blooded animals, five jaws, belonging to at least two species of a small animal resembling the didelphys, or opossum, and about the size of a mole, have been found in the calcareous slate of Stonesfield, in Oxfordshire, a stratum considerably below the chalk. One fact of this kind is of more value than a host of negative evidence, and it is triumphantly appealed to by those geologists who contend for the uniformity of the course of nature from the earliest epochs to the present time. It is evident, they say, that some of our oldest rocks have been derived from the waste of pre-existing land; and, as that land appears to have been clothed with its appropriate vegetation, we have no right to suppose that it was destitute of its appropriate animals. A great ocean like the Pacific, interspersed, like it, with small islands appears to have prevailed, during the formation of the older strata, over that part of the Northern Hemisphere in which are situated those countries whose geology has been most explored. Small oceanic islands do not, at the present day, contain many mammalia, while they are wholly destitute of the larger kinds; and the discovery of such remains, in an oceanic sediment, after its conversion into dry land, must be an event of very rare occurrence, for however abundant mammalia might be on some distant continent, by the rivers of which their carcases would be drifted down, yet before they could be floated out far to sea, they would be almost certain to be devoured by the carnivorous monsters of the deep; and even supposing them to escape this fate, the chances are very much against the discovery of the spot where these rare remains are concealed after the bed of the ocean shall be laid dry.

Allowing to this argument its due weight, we must observe, that there are certain tribes of mammalia (as the whale and the seal) which are peculiar to the sea; and the imbedding of their remains in the sediment of a great ocean would be no uncommon event. Yet no traces of their remains have hitherto been found in the strata below the chalk*. The absence therefore of the remains of marine, as well as of terrestrial, mammalia, appears favourable to the conclusion, that this order of animals was not created until an epoch comparatively recent, though that epoch is proved, by the Didelphoid remains of Stonesfield, to have been more remote than we formerly supposed. We must, likewise, observe that there is a local deposit, older than the chalk, but more recent than the Stonesfield slate, principally confined to the Weald of Kent and Sussex, and thence named the Wealden, which, from its assemblage of fossils, (derived, with few exceptions, from land and fresh water) appears to have been deposited at the mouth of a river; and, from the extent of the formation, that river must have drained some large continent. Here, then, we ought to find the remains of mammalia if that class of animals were as abundant in the world then as they were at a subsequent period. But though this deposit has long been under the examination of an eminent geologist, celebrated for his skill in comparative anatomy, not a trace of such remains has yet been discovered. On the contrary, the Wealden exhibits a most curious group of animals as inhabiting the shores and waters of the river which drifted down their remains, and indicates an extraordinary developement, during that era, of the saurian, or lizard family. They seem to have possessed earth, and sea, and air. Inhabiting the sea, there was the ichthyosaurus, a creature intermediate between a crocodile and a fish,—having the snout of a dolphin, the head and teeth of a crocodile, the doubly concăve vertebræ of a fish, fitting it for rapid motion through the water, and, instead of fins or feet, it had four paddles, like those of a turtle; while nearly allied to it in structure was the plesiosaurus, one species of which had a long neck like the body of a serpent. These creatures appear, from their structure, to have been confined to the sea, and never to have appeared on dry land; but there were amphibious saurians, of the crocodile and alligator families, frequenting the rivers and estuaries, in company with tortoises and turtles; and there were pterodactyles, or flying saurians, together with a great variety of gigantic terrestrial saurians, the lord of whom appears to have been the iguanodon, a creature pronounced by Cuvier to be the most extraordinary that had ever come under his consideration. It was an herbivorous lizard, of colossal proportions, for it must have been seventy feet long, and nine feet high. In some respects its teeth bore an analogy to those of the iguana, a small species now inhabiting the West Indies; but it differed from all other herbivorous lizards, either fossil or recent, in having chewed its food, as appears by the manner in which its teeth were worn, whereas other herbivorous lizards nip or crop theirs, and swallow it without mastication. In short, during the Wealden era, there were lizards twice the size of the largest elephant or rhinoceros, approaching them in the character of their dentition, and apparently occupying their place in the economy of nature.

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* The bones found in the Wealden, and described by Dr. Buckland as belonging to Cetacea, are referred by Cuvier and Mantell to some unknown Saurian.

But whether mammalia existed during the earliest epochs of the world of which we possess geological monuments, and were contemporary with those marine animals imbedded in the lower strata, though, for the reasons above stated, their remains have not yet been discovered; or whether they were not created till a later period, though still before the deposition of the chalk, it is certain that during the tertiary era, when their remains were abundantly entombed in the strata, we can trace the introduction of new races even of them. The remains of the land quadrupeds imbedded in the olden tertiary strata, are exclusively those of extinct *genera*. In the deposits of a more recent period we meet, in these northern latitudes, with the remains of extinct *species* of genera now existing, but existing only in warm climates, such as the elephant, rhinoceros, hippopotamus, &c. These are found to be gradually intermixed (in the ascending order of the strata) with the bones of mammalia, identical in species with those now living under the present climates of Europe; till at length we come to peat bogs and alluvial deposits, in which human remains occur, mixed with those of animals now living in the countries where the remains are found, together with a few which have become locally extinct, within the historic period. The animals that have become locally extinct in Britain are, the wolf, the beaver, and the wild boar. The great fossil elk of Ireland, now universally extinct, perhaps continued to exist in that country after it was inhabited by man. It is doubtful whether the elephants, hyænas, tigers, &c., whose remains are so abundant in gravel and in caves, inhabited Europe after the human era. In those cases where their bones occur in caverns, mixed with the bones and works of men, there is often evidence that the latter were introduced after the former; and in no case is there clear evidence that they were deposited simultaneously. It is a very remarkable circumstance, that not a single bone of any of the monkey tribe has yet been found in a fossil state, though so many bones of elephants and other inhabitants of the same climate as the monkey, have been disinterred in Europe. This would almost lead to the conclusion that the class of animals most resembling man in organization were created nearly at the same time with him. It is needless to adduce proofs of the recent introduction of man upon the earth. It is a fact admitted by all geologists, even by those who contend that the existing order of things is but the last link of a chain of events derived from laws now in daily operation. The most able and strenuous advocate of this doctrine asserts, that the real difficulty consists in tracing back the signs of man's existence upon the earth to that comparatively modern period when species, his contemporaries, began to predominate.

The absence of human remains from the older strata cannot be accounted for as in the case of the quadrupeds of the land, by the rare occurrence of the circumstances which would cause them to be imbedded in submarine sediment. "No inhabitant of the land," says Mr. Lyell, "exposes himself to so many dangers on the waters as man, whether in a civilized or a savage state; and there is no animal, therefore, whose skeleton is so liable to become imbedded in lacustrine and submarine deposits: nor can it be said that his remains are more perishable than those of other animals; for in ancient fields of battle, as Cuvier has observed, the bones of men have suffered as little decomposition as the bones of horses, which were buried in the same grave. But even if the more solid parts of our species had disappeared, the impression of their form would have remained engraven on the rocks, as have the traces of the tenderest leaves of plants, and the soft integuments of many animals. Works of art, moreover, composed of the most indestructible materials, would have outlasted almost all the organic remains of sedimentary rocks. Edifices, and even entire cities, have, within the times of history, been buried under volcanic ejections, submerged beneath the sea, or ingulfed by earthquakes; and had these catastrophes been repeated through an indefinite lapse of ages, the high antiquity of man would have been inscribed in far more legible characters on the frame-work of the globe, than are the forms of the ancient vegetation, which once covered the islands of the northern ocean, or of the gigantic reptiles which, at still later periods, peopled the seas of the northern hemisphere*."

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* *Lyell's Principles of Geology*, vol. i., p. 241.

It can scarcely, we think, be denied that the facts above enumerated point to something very like a progressive developement of organic life; though they will not support the doctrine to the extent contended for a few years ago, and though the terms in which it was announced might be open to objection. It is possible to repudiate the monstrous and absurd notions of Lamarck, respecting the transmutation of one species into another,—it is possible to admit that fishes, reptiles, and mammalia occur, respectively, in older strata than was supposed at the time Sir Humphry Davy wrote his *Consolations in Travel*,—we may admit the absence from the coal strata of fungi, lichens, and mosses, which are the simplest forms of flowerless vegetation, and the presence of ferns and Lycopodiaceæ, which are the most highly organized of the cryptogamic* plants,—we may admit that the monocotyledons† of the same period consisted of the most highly-developed of that class of plants, and that it was not destitute of its dicotyledons‡. It may be true that an orthoceratite or a nautilus, though they have no back-bone, are, for the purposes for which they were designed, as perfect in their organization as an elephant or a crocodile; and that palms and bread-corn, though they have but one seed-lobe, are not inferior in dignity to an oak and a nettle which have two: and yet, admitting all this, we may contend with Professor Sedgwick, that a doctrine may be abused and yet contain the elements of truth,—that it is one thing to refute it, and another to point out the errors and overcharged statements of its supporters. "With reference," he says "to the functions of the individual, one organic structure is as perfect as another; but I think that in the repeated, and almost entire, changes of organic types in the successive formations of the earth,—in the absence of mammalia in the older, and their rare occurrence (and then in forms entirely unknown to us) in the newer secondary groups,—in the diffusion of warm-blooded quadrupeds (frequently of unknown genera) through the older tertiary systems,—in their abundance (and frequently of known genera) in the upper portion of the same series,—and, lastly, in the recent appearance of man on the surface of the earth, now universally admitted,—in one word, from all these facts combined we have a series of proofs, the most emphatic and convincing, that the existing order of Nature is not the last of an uninterrupted succession of mere physical events, derived from causes now in daily operation, but on the contrary, that the approach to the present system has been gradual, and that there has been a progressive developement of organic structure, subservient to the purposes of life§."

While these changes were taking place in the organic world, changes as great were taking place in the inorganic. Land was converted into sea, and sea into land, and land again into sea. The same portions of the earth's surface have been subject to repeated oscillations, so as to be alternately above and below the ocean level. We have already alluded to the indications of a change of climate, discoverable in the crust of the globe. We shall not at present enter into a detail of all the evidence on which this rests; suffice it to say, that from the testimony of the imbedded organic remains, it appears that, during the formation of the older strata, a tropical, if not an ultra-tropical, climate prevailed over the northern hemisphere, and that even at a period so comparatively recent as the commencement of the tertiary series, a very high temperature existed over what are now the cool regions of Europe; when a great part of England was yet beneath the waves, when her mountain-chains formed a cluster of spice islands, haunted by the crocodile and the turtle, and when, on the spot where London now stands, the nautilus of the tropics, spreading his sail to the breeze, was the only representative of the fleets that crowd her port. But if the earth's crust furnishes us with evidence, that great and repeated changes have taken place in the organic and inorganic world, it furnishes us with proofs no less clear, that great epochs of time elapsed while these changes were in progress,—epochs so great that we are tempted to connect them with the secular periods of astronomy to which we have before alluded. There is, however, this difference between the phenomena of astronomy and geology; that in the former we have a series of events recurring, in a fixed order, after the lapse of fixed intervals of time, whereas in geology (if we except the interchange between land and sea, and the recurrence of volcanic action after long intervals of repose) we have no evidence of the repetition of a single phenomenon, much less have we evidence of geological cycles, in which the same events are repeated, again and again, in a stated order, and at stated intervals. We have proofs of a change from a hot to a colder climate; but we have no proofs of a change from a cold climate to a hotter. Whole orders of fishes characteristic of the older strata become extinct, and are succeeded by new races, which in their turn give place to others. This class of vertebrated animals affords an unbroken record, from the earliest to the most recent geological epoch, and the changes which occur in it are more rapid than those which take place among invertebrate animals; but, as we ascend in the series of strata, we meet with no instance of the revival of any of the extinct genera or species. By means of a series of geological monuments, we can trace the commencement and decay of the family of the Ichthyosauri; but once extinct they reappear no more, except in the humorous sketch of Mr. Delabeche*: and, lastly, man becomes an inhabitant of the earth, but there is not a particle of evidence that the race had previously existed, though at some very remote period, and had been destroyed to revive on the completion of a great geological cycle. Again, to use the language of Professor Sedgwick, "each formation of geology may have required a very long period for its complete developement: but, after all, the successive formations about which we speculate, however complex in their subdivisons, are few in number; and after deciphering a series of monuments, we reach the dark ages of our history, when having no longer any characters to guide us, we may indulge at will in the creations of our fancy. We may imagine indefinite cycles, and an indefinite succession of phenomena, and in the physical world as well as in the moral, we may have our long periods of fabulous history. But these things belong not to inductive geology; and all I now contend for is, that in the well-established facts brought to light by our investigations, there is no such thing as an indefinite succession of phenomena*."

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* Plants whose fructification is concealed.

† Plants having only one seed-lobe.

‡ Plants having two seed-lobes.

§ Sedgwick's Anniversary Address.

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* Allusion is here made to a lithographic sketch from the pencil of that gentleman, entitled, "Reappearance of Ichthyosauri—man only found in a fossil state;" in which Professor Ichthyosaurus is represented lecturing to an audience of his brethren, on a human skull, which he pronounces to belong to one of the inferior animals, on account of the insignificance of the teeth, and the trifling powers of the jaw, expressing wonder how the creature procured its food.

When the Copernican system of astronomy was established, and the earth, no longer regarded as the centre of the universe, was proved to be one of a system of bodies revolving round the sun, the question naturally arose were the other bodies of that system habitable and inhabited; and, reasoning from analogy, astronomers were disposed to answer the question in the affirmative. It was true, that as regards the distribution of light and heat, and the intensity of gravitation, a very different state of things must prevail in most of those planets from what obtains on the earth; but seeing that every part of this earth was crowded with sentient beings, possessing an organization so adapted to the conditions of existence assigned them, as to render that existence a state of enjoyment, it appeared highly improbable that all the variety here displayed should be limited to one planet, and that all the others should be mere blanks, made only to be gazed at by us, and destitute of beings suited to their respective states. And when the modern researches of astronomy extended to the fixed stars, showed them to be suns, like that which forms the centre of our system, and when it was found that they were arranged in groups circling round each other, the argument from analogy was carried further, and the probability was inferred of each of these suns being attended by its system of planets, with their satellites, the whole teeming with life under a countless variety of forms, and under a countless variety of conditions. Geology comes in aid of these conjectures, by showing that this our planet has existed under a different distribution, of land and sea, of heat, and perhaps of light, from that which it at present enjoys; and that, under these different circumstances, it was not a mere blank, but was as much crowded as now with living beings adapted to the then state of things. A contemplation of the variety of organization manifested in the world around us, cannot fail to excite our wonder; but if we extend our researches to the remains entombed in the earth's crust, our wonder will be increased at the increased variety we shall find; and we shall be convinced that the existing system of nature is but a part of what has been, and that the whole visible creation, past and present, may be but an atom compared with the invisible.

If we apply ourselves to the task of classifying organized bodies now existing, arranging them in groups as they differ from or resemble each other in their structure, we find that those forms of each group which are most dissimilar, are connected by a series of gradations, separated from each other by the most minute distinctions, and that the groups, whether we regard the larger or the subordinate divisions, are again connected by forms possessing some of the characteristics of two groups. There are, however, cases in which the transitions are more abrupt; and when these cases occur we frequently recover, among the extinct forms of an ancient state of nature, those connecting links which are wanting in the existing creation. As instances, we may mention the ichthyosauri, before alluded to, as combining some of the characters of a lizard with those of a fish, and occupying among saurians the place of the whale and seal among mammalia; and pterodactyles which bore the same analogy to lizards, that the bat now bears to mammalia. The pachydermatous* order of mammalia, as existing at present, consists of but few genera,—the elephant, rhinoceros, hippopotamus, horse, hog, and tapir, genera possessing but slight resemblance to each other, and singularly poor in species. The tertiary strata of the Paris basin abounds, however, in remains of extinct animals of this order, supplying gradations between some of the above genera, and connecting this order with others. There are, in particular, above forty fossil species of one division of it which contains only four living species. Similar instances might be adduced from other divisions of the animal kingdom; and others are afforded by a comparison of fossil and recent vegetation.

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* Anniversary Address—*Proceedings of Geological Society*, p. 305.

Thus existing and extinct organic bodies can scarcely be said to belong to different systems, but must be considered as composing one great chain of being, formed on one general plan, agreeing, for the most part, in important points of structure, and differing only in minutiæ of detail. And if, on examining the organization of beings whose functions we understand, we discover a mechanism like the work of our own hands, but far surpassing in beauty, and excellence, and complexity any workmanship of man: and if in this we see proofs of contrivance, of structure designed for an end, and that end accomplished, we not only discover among fossil organic bodies new and unexpected instances of this, but we have proofs that the same contriving Intelligence has been exerted at the remotest period to which we can trace back the history of the globe, ages, we know not how long, before the existence of the human race.

We find, moreover, that with every change in the state of the earth there has been a corresponding change of organized bodies, and thus we have proofs, not only of an Intelligence adapting mechanism to an end, but that the same contriving Intelligence has been manifested at successive periods adjusting the mechanism to the altered conditions under which it was to exist. With what reverential ideas ought these views to impress us respecting that Being to whom all things owe their existence, eternal in duration, absolute in power, perfect in wisdom, in goodness infinite; and how can we refrain from exclaiming in the language of Holy Writ, "O Lord, how manifold are thy works, in wisdom hast thou made them all: they shall perish but Thou shalt endure; yea, all of them shall wax old as doth a garment, and as a vesture shalt Thou change them, and they shall be changed, but Thou art the same and thy years have no end."

We have now pointed out some of the leading facts which geology presents to us, and some of the lofty speculations to which they lead, and we have compared them with those of astronomy. Before we proceed to the details of the science, we shall offer a few observations on the advantages attending the study, and we shall endeavour to remove those scruples which cause many well-meaning persons to view it with an unfavourable eye, as opposed to facts recorded in the Sacred Writings.

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* Thick-skinned.

THE SALT MOUNTAINS OF ISCHIL.

My object in turning out from the direct road to Salzburg, was to visit the Gmunden Lake and the salt mountains of Ischil, which I was told I should find on its southern shore. The road, after leaving Lambach, led by the Traun river, by which the lake discharges itself into the Danube, and down which the salt is conveyed in large flat-bottomed boats. It is an extremely turbulent and rapid stream, and at one spot, where is a high picturesque fall, a wood-shot has been constructed, down which the boats glide with frightful rapidity. At the head of the lake is the village of Gmunden, where is a depôt for salt, and a large manufactory of casks, both belonging to the government. I found here two German students, also pedestrians, with whom I kept company on to Salzburg. We hired a boat to convey us to the other end of the lake. It was rowed by two men and a girl, there being scarcely any kind of manual labour from which the females of the lower class in Germany are exempted. The lake is romantically situated, having on the eastern side a range of mountains rising boldly from the water, and on the other a champaign country highly cultivated, and sprinkled with *herrschaufts** and farm-houses. Among the former was the Traun Stein, which rises abruptly from the lake about two miles from its outlet. It was a bright morning; every object looked cheerful, and my companions when out on the lake commenced a song about *freiheit* and *vaterland*, to an air that I had often heard among the Germans of my own father-land Pennsylvania. Suddenly the boat stopped, and the father of the crew, rising up, sprinkled us with water, and with the usual ceremony of baptism gave us each the name of one of the surrounding mountains.

We landed at Ebens-see, a small village at the southern end of the lake, and in reply to our inquiries, they informed us that the salt was manufactured at this place, but that the salt-mines were several miles in the interior. I had supposed that the salt was dug in a solid state from the mountain, and was therefore surprised when they took us to a large building, in which was a sheet-iron pan about sixty feet in diameter and two in depth, with a brisk fire kept up beneath. Water was flowing into it from two huge cocks, and workmen were employed shoveling salt out from the bottom on to a draining-board, from which it was afterwards removed to small cone-shaped vessels, with holes at the bottom for further draining. In these it was suffered to remain until it became solid, when it was turned out, and the moist end of the cone being cut off, it was ready for transportation. Each lump contained about thirty-three pounds.

From Ebens-see we followed the windings of a deep valley for nine miles, when we arrived at Ischil, a pretty little village, frequented by valetudinarians for the benefit of its salt-baths. These are in a new and very handsome edifice, with a Grecian colonnade in front, and an inscription, *In sale et sole omnia existunt.* The salt mountains are about three miles to the southward of Ischil. They form part of a high and broken range extending eastward and westward, and in the exterior are not to be distinguished from other parts of the range, the vegetation on every part being equally luxuriant. About half way to the summit, we arrived at the residence of the superintendent, and having here obtained permission to enter the mines, were conducted to a house a few hundred yards below, and provided with suitable dresses. Here is one of the entrances, of which there are twelve in all: they informed us that salt is found in any part of the mountain where they take the trouble of digging for it. Our course after entering was along a narrow horizontal gallery, openings occurring at intervals, along which we heard the dashing of water: at our feet were also wooden pipes for water, with branches running off into the various lateral galleries. Having proceeded a quarter of a mile, we came to a halt just where some bare logs rose in a slanting direction, from a cavity whose depth we could not ascertain. A guide straddled this log, and directing me to do the same, and hold on by him, he raised his feet, and away we went, sliding, or rather darting down on the smooth log, and, excepting the glimmering light from our lantern, enveloped in total darkness. The guide kept himself upright, and holding fast to him, I presently found myself deposited in safety on a heap of soft earth, and turned to enjoy the equal astonishment and fright of my companions.

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* *The residences of the titled proprietors.*

We were now at the bottom of a chamber of irregular shape, but averaging about one hundred and fifty feet in diameter, and from four to ten feet in height; the ceiling in some parts being supported by blocks of sulphate of lime, piled up in the form of rude columns. The *gangue* of the salt, if the word may be used, is composed chiefly of a clayey earth, mixed up with irregular blocks of sulphate of lime: the salt is mingled with these, usually in strata of from six inches to two feet in thickness, forming, however, every variety, shape and direction. It was generally of a reddish colour, and though mixed with impurities, very strong. The strata were very distinct on the ceiling of the chamber, which looked not unlike marbled paper, the salt itself presenting a great variety of colours, and its gangue scarcely a smaller number. The surface of the salt presented to us was rough and honey-combed.

We now for the first time learnt the mining-process, which certainly is very simple, and sufficiently economical. In the first place, a small chamber is formed by the pick-axe and shovel, and arrangements having been made by means of pipes for conducting water to and from it, the outlet is stopped up, and the chamber is filled with fresh water, of which the mountain-streams furnish them with abundance. In a few weeks the water in the chamber is saturated with salt; it is then let out, and conducted by aqueducts to Ebens-see, a distance of twelve miles, where, as I have already described, the water is evaporated artificially, and the salt is shipped for the store-house at Gmunden. When the chamber has become sufficiently dry, the workmen descend into it, clear it from the stones and dirt which have been loosened by the water and fallen from the ceiling, and the chamber is then ready for another flooding. The large chamber we were in, as the guides informed us, requires one month for the process of filling, fifteen days more for completing the saturation: it holds eighty *thousand* German Emers, is filled four times a *year, and has been in use thirty* years: one hundred lbs. of water furnish twenty-six and three-fourths lbs of salt. There are thirty-four chambers in all, in which two hundred men are employed, working night and day, six hours at a time. They work four days in a week, and get forty-eight cents per week. When the chambers are approaching so as to threaten a breach from one into the other, the further encroachment of the water in that direction is prevented by a compound formed by the clay and pulverized rock, which is beaten against the wall so as to form an effectual barrier. At intervals, in the descent of the mountain, are three reservoirs, into which the water is successively discharged, I believe for the purpose of breaking the violence of the descent.

There is a chain of six or seven very beautiful lakes in this neighbourhood, two of which we visited after leaving Ischil, and on the 29th August stopped for a short rest at Salzburg. Our consul at Vienna had described in glowing terms the beautiful scenery at Berchtsgaden, a short day's journey to the south of Salzburg, and as it had also a salt-mountain, I determined to pay it a visit. There are also salt-mines at Hallein, south from Salzburg, which I did not examine, but which I was informed are worked, and are about as productive as those of Ischil.

Berchtsgaden is now comprehended in the kingdom of Bavaria. The royal family were there on a visit at this time: they had just been inspecting the mines, and I found many parts of the interior ornamented in a fanciful manner; the richest crystals of the salt and gypsum having been collected and disposed so as to form grottoes, devices, &c.: some of the former were large and perfectly transparent, but a deep red or brown is the prevailing colour. This mine appeared to me to be richer than that of Ischil. In some parts the salt forms regular solid strata, several feet in thickness, and so free from foreign matter as to be fit for use without any purifying process. In these places it is mined by the aid of gunpowder, and the guides, after placing us in secure places, allowed us to witness two or three explosions. Generally, however, the mine differs very little from that of Ischil. We entered by a horizontal gallery, a quarter of a mile in length, and then came to branching galleries, along which pipes were conducted for filling the chambers with water, or emptying them. One hundred and ninety men are employed, and the yearly product, I was told, is eight thousand one hundred and thirty-four tons.

[*From* SILLIMAN'S *Journal.*]

ACCOUNT OF THE UNROLLING OF AN EGYPTIAN MUMMY,

WITH INCIDENTAL NOTICES OF THE MANNERS, CUSTOMS, AND RELIGION, OF THE ANCIENT EGYPTIANS.

BY T. J. PETTIGREW, F.R.S., F.S.A., &c.

ONE of the Friday evenings so judiciously set apart by the managers of the ROYAL INSTITUTION for the communication of useful knowledge, has lately been devoted to the consideration of the mummies of the Ancient Egyptians, and a lecture upon this subject was delivered on the 27th of May, by Mr. Pettigrew, whose attention has been frequently directed to this interesting subject, and the result of whose researches have been submitted to the public*. The mummy unrolled upon this occasion was one purchased by Mr. P. at a late sale of Egyptian antiquities, consisting of the collection made by the late Mr. Salt, his Britannic Majesty's Consul in Egypt. The lecturer stated that he had availed himself of the opportunity afforded by the Royal Institution to display the mummy to the members and their numerous visitors, in obedience to the solicitations of several distinguished scholars and antiquarians, and had overcome his own scruples in appearing before his audience in a public character, believing, as he did, that such an effort might be found useful in exciting a spirit of inquiry into an object of interest connected with Egyptian antiquities, and calculated to prove advantageous in assisting to demonstrate the certainty of hieroglyphical research, which he regretted to say, notwithstanding the great advances made in the course of a few years, had not yet, in the minds of many, sufficiently established its claim to truth and authenticity, and was still treated as being almost wholly speculative and conjectural. Mr. P., therefore, felt peculiarly happy in the present opportunity of putting this subject to the test, in the presence of nearly all those† who have been in any way distinguished for their knowledge of hieroglyphical literature, and whose labours have tended so much to illustrate this curious branch of literary history; and to whom he was willing, on the present occasion, to submit the translation he had made of some of the principal passages depicted on the cases of the mummy, feeling satisfied with the decision of such competent judges; and deeming it of importance to show to those who may be sceptical on the matter, that when so many persons of known attainments can be brought together to agree as to the interpretation of the inscriptions, the basis upon which they are made must necessarily and obviously be founded in truth; and, therefore, likely to lead to important and correct results. With these preliminary observations, Mr. Pettigrew proceeded to consider the subject. He abstained from entering into any consideration of the etymology of the word MUMMY, whether it was to be traced to the Arabic noun *mum*, signifying wax, or *amomum*, a kind of perfume, as Salmasius describes it; but stated that he should employ the term on the present occasion as applying to the body embalmed, and not to the embalming ingredients.

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* *History of Egyptian Mummies, and an Account of the Worship and Embalming of the Sacred Animals by the Egyptians, &c.* 4to. Longman and Co., 1834.

† Mr. Wilkinson, Mr. Burton, Dr. Leemans, Lord Prudhoe, Rev. Mr. Tattam, Mr. Cullimore, &c.

Mummy, it is well known, was, during the sixteenth century, extensively used in medicine, and there were not wanting authorities of high character to vouch for its healing properties. Avicenna, Serapion, and others among the Arabian physicians, spoke loudly in its praise; and in more modern times Lord Bacon, Robert Boyle, and others, equally eulogized its virtues. Lord Bacon declares that "Mummy hath great force in staunching of blood; which, as it may be ascribed to the mixture of balmes that are glutinous, so it may also partake of a secret propriety, in that the blood draweth man's flesh*." And Boyle says it "is one of the useful medicines commended and given by our physicians for falls, and bruises, and in other cases too†." Its introduction is attributed to a Jewish physician named Elmagar, a native of Alexandria, who, it is said, was, as far back as the fourteenth century, in the habit of prescribing it to the Christians and to the Mahometans then in the East, contending for Palestine. The demand for it gave rise to the spurious manufacture of mummies, and the traffic ceased in consequence of the importunities of a Jew engaged in these practices at Damietta, who, anxious to convert a Christian slave to the Hebrew faith, proposed to him as an evidence of the sincerity of his conversion, to submit to the ancient rite of circumcision. To this the boy objected, and was subsequently ill-treated by his master, whom he therefore denounced to the pacha, by whom the Jew was thrown into prison, and fined in 300 sultanins of gold; this being readily paid, the governors of other provinces, Alexandria, Rosetta, &c., levied a ransom upon all engaged in the commerce of mummies, and the Jews, to avoid a new oppression, discontinued their trade. Thus did mummy cease to be an article of the Materia Medica, and not from any discovery of its inefficiency in the cure of disease, to prove which, however, the celebrated Ambrose Paré published a treatise.

The desire of immortality, Mr. P. observed, is most strongly rooted in the mind of man, and innumerable means have, in all ages, and under all circumstances, been devised to perpetuate his memory after death. In no part of the world does this principle appear to have acted so strongly as in Egypt; and its ancient inhabitants have not only built huge pyramids, and erected mighty temples and obelisks, covered with symbols, expressing, in hieroglyphical language, characters and events; but they have succeeded beyond all other ages and people, in preserving from decay the remains of their own fragile frames. The extraordinary perfection to which they carried the art of embalming the dead is, perhaps, alone to be accounted for by referring to their theology. Believing in the immortality of the soul, the ancient Egyptians conceived that they were retaining the soul within the body as long as the form of the body could be preserved entire, or were facilitating the reunion of it with the body at the day of resurrection, by preserving the body from corruption; and Herodotus says, "The Egyptians are the first that laid down the principle of the immortality of the human soul; and that, when the body is dissolved, the soul enters into some other animal which is born *at the same time;* and that, after going the round of all the animals that inhabit the land, the waters, and the air, it again enters the body of a man which is then born. This circuit, they say, is performed by the soul in 3000 years*."

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* *Sylva Sylvarum,* Cent. X., s. 980.

† Boyle's Works, Vol. II. p. 451.

There is abundant testimony as to the antiquity of the practice of embalming; but the information possessed as to the period of its cessation is but scanty. We have, however, the evidence of St. Athanasius and St. Augustine upon the subject, and it is clear that it was practised as late as the early part of the fifth century. The practice is distinctly alluded to by these fathers of the Church, who tell us that the Egyptians exercised a process by which the bodies were rendered as durable as brass; and St. Augustine says, the Egyptians had a better idea of the resurrection of the body than any other people. "Egyptii soli credunt resurrectionem, quia diligenter curant cadavera mortuorum: morem enim habent siccare corpora et quasi ænea reddere, *Gabbaras* ea vocant†."

Among the ancient authors to whom we are indebted for accounts of the processes of embalming, must be enumerated Herodotus, Diodorus Siculus, and Porphyry. Herodotus and Diodorus Siculus describe, with most precision, the several modes of embalming; but Mr. P. stated, that it was impossible to reconcile all the kinds he had even witnessed to the division laid down by these ancient authors. Still it must be confessed, that their authority is of the greatest importance, for there is no one circumstance mentioned by them as belonging to the process of embalming which is not to be found, though perhaps not precisely in the order in which it is described. The time devoted to a lecture is insufficient to dwell upon these particulars; it would merely permit the lecturer to state, that *three* principal modes‡ had been enumerated, and these he briefly referred to, in order that the kind which had been selected in the mummy, to be unrolled on this occasion, might be the more readily detected.

In the *first*, the most expensive, the brain was extracted through the nostrils by the aid of bronze crochets, and injections of bitumen were afterwards thrown into the skull. An incision was made into the left flank, from which the intestines were removed, then washed with palm wine, sprinkled with aromatics, and either returned into the body or placed in vases called *Canopic*. The body was then steeped in a solution of natrum for seventy days, at the expiration of which time it was washed, bandaged in cloths, and deposited in cases.

In the *second* mode, the brain was not extracted, no incision was made into the flank, but the intestines were withdrawn *per anum;* after which injections of oil of cedar and of natrum were thrown into the body, and it was steeped in natrum for the specified time. Little beside skin and bone remaining, they were given to their friends without any further operation. In the *third* mode the inside was washed with a liquor called *surmaia* (an infusion of senna and cassia), the body placed in natrum, and afterwards delivered to the friends.

Diodorus Siculus is particular in his relation respecting the funeral ceremonies of the Egyptians. He tells us* that the expense of the first mode of burial was a talent of silver, (two hundred and twenty-five pounds English money); the second, twenty minæ, (seventy-five pounds English); and the third, a trifling sum not mentioned. But the mode being selected, the friends of the deceased leave the body with the embalmers to be prepared. The body being laid down, the scribe marks the place for the incision on the left flank, which is then made by the cutter, or slitter, with an Æthiopic stone. Having done this he flies away as fast as he can, for even his companions in the process of embalming pursue him, cast stones at him, and curse him, the Egyptians holding that whoever offered violence, wounded, or in any way injured a body of the same nature as himself, deserved their hatred. The embalmers they esteemed worthy of honour and respect, and we learn that they were familiar with the priests, and went into the temples as holy men, without any prohibition. Mr. P. inferred from this statement that they were an inferior kind of priesthood. The incision being made, the embalmer removes the intestines and other viscera, all except the heart and kidneys. The intestines, as has been stated, are washed with palm-wine, and then sprinkled with aromatics, sometimes returned into the body, and sometimes placed in canopic vases. Porphyry† has handed down to us the prayer in the name of the deceased, uttered by the embalmers on this occasion. It is as follows:—"O thou Sun, our lord, and all ye gods, who are the givers of life to men! accept me and receive me into the mansions of the eternal gods; for I have worshipped piously, while I have lived in this world, those divinities whom my parents taught me to adore. I have ever honoured those parents who gave origin to my body, and of other men I have neither killed any, nor robbed them of their treasures, nor inflicted upon them any grievous evil; but if I have done anything injurious to my own soul, either by eating or drinking anything unlawfully, this offence has not been committed by me, but what is contained in this chest," meaning the intestines, which, according to this author, are then thrown into the river. The injections into the body, and the application of the cedria and other substances, complete the process.

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* Herodot. *Hist.* lib. ii. s. 123.

† *Sermones* cxx. *de Diversis, cap.* 12.

‡ *To those of our readers who may be anxious for more particular information* on this head, we must refer to Mr. Pettigrew's *History of Egyptian Mummies*, where the several parts of the different processes are minutely detailed.

According to Herodotus, Diodorus Siculus, and other historians, the ancient Egyptians were a people holding truth and virtuous conduct in the highest estimation. Their penal laws applying to cases of homicide, parricide, perjury, adultery, rape, &c., mark the high sense of justice entertained by them, and this is even carried to the verge of the tomb; for we learn from Diodorus Siculus‡, that upon the death of any one, the relations of the deceased were obliged to announce to the judges (assessors forty-two in number) the time at which it was intended to perform the ceremony of burial. This consisted, in the first place, of the passage of the deceased across the lake or canal of the department, or nome, as it was called, to which the deceased had belonged. The jury being named, the assessors assembled, and the court of inquiry was open to all, so that any accusation might be urged against the defunct. Should his life have been bad, the right of sepulture was denied to him, which was considered as one of the greatest calamities that could occur. If, on the contrary, the life of the deceased had been well-conducted and blameless, and that no reproach could attach to his memory, an eulogium was pronounced upon him, and he was permitted to be entombed with all due honour. Diodorus Siculus informs us, that in these eulogies no mention was ever made of the race or family of the defunct, all the Egyptians being considered equally noble. No one was exempt from this ordeal,—kings as well as the ordinary people were subjected to the same inquiry,—those who during life no one dared to reproach, or whose actions no one dared to question, when dead were submitted to a rigorous examination. A public audience was given to hear all accusations against the deceased monarch. The priests commenced by making his eulogy, and recounting his good actions. If the general opinion of the people as to the government and conduct of the monarch corresponded with that of the priests, the multitude poured forth their acclamations; but if the contrary, murmuring succeeded: and Diodorus Siculus says, there have not been wanting instances of the denial of burial to a deceased sovereign, in accordance with the decision of the people. M. Champollion saw in Biban-el-Molouk, the tomb of a king, in which the sculpture had been defaced from one end to the other, except in those parts where were sculptured the images of the queen, his mother, and of his wife, which have been most religiously respected, as well as the hieroglyphical legends relating to them. M. Champollion conceives this to have been the tomb of a king, condemned by the judgment after his death, and denied the rite of burial[*].

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* *Biblioth. Histor.*, lib. i. s. 91.

† *De Abstinentia*, lib. iv. cap. 10.

‡ Lib. i. s. 92.

Mr. Pettigrew illustrated this part of his subject by reference to some drawings, and to papyri, that had been found within the cases enclosing the mummies. He exhibited drawings of three funeral boats, which are exceedingly curious; two of these were lately purchased, at a very high price, for the British Museum; the third, discovered by Signor Passalacqua, in 1823, in a family sepulchral chamber, in the Necropolis of Thebes, is in the collection belonging to the Prussian government, at Berlin. These boats are all of similar construction, but the latter appeared to be the most complete, and was therefore particularly described. The boat, or bari, is cut out of sycamore-wood, and measures two feet, eight inches, and six lines, French measure. The boat is furnished with a large projecting portion of wood at the prow and at the poop. In the centre is a male mummy extended on a sofa, or table, of which the legs are formed of the limbs of a lion; this is surmounted by a canopy, on which are inscribed various hieroglyphical characters, and is supported by six pillars, painted successively in red, black, white, and green. At the head and feet of the mummy are two female figures; one of them is in an attitude of great grief and desolation, represented by the hair of her head falling upon the mummy, whilst her arms are employed in embracing the deceased; the hands of the other are placed upon the feet of the mummy. Four priests are seated upon the deck of the vessel, one at each corner of the table, or bier, whilst another in front is observed to be holding out a MS. unrolled before him, and appears to be delivering a funeral oration. Another, a sacrificer, is, with knife in hand, prepared to immolate an ox, which lies bound at his feet. The first figure on the prow has his right arm extended, and appears to be watching the course of the vessel. The pilot, who, from his long white tunic, may be supposed to be a priest, is seated at the poop between the two oars, the mechanism for moving which is very curious. The oars and the pillars on which they move are crowned with the head of a hawk. The body of the vessel is of a green-colour, the extremities of a deep-blue tinge. Paints and frames, to serve in the representation of religious ceremonies, are lying on the vessel, and at the sides of the fore-part are emblematical representations of the sacred eye, the eye of Osiris, which is also represented on the fore-part of the oars, surrounded by leaves of the lotus. The plank to descend from the vessel, and the pegs to fasten it, together with the club to drive them into the earth, are also on the deck. The male figures in the vessel have a red tinge of countenance, the women a yellow one, which corresponds with what is commonly seen in the ancient Egyptian paintings. It is to be observed that the priests, as well as the females, have their heads well covered with hair, which was permitted to grow during the term assigned for the mourning for the dead. This induced Signor Passalacqua to conceive the funeral ceremony here represented to be that of a priest, and the hieroglyphical inscription on the MS. reads, according to M. Champollion, "Grand-Prêtre." In the tomb, in which three coffins or cases, enclosed one within another, were found, these hieroglyphical symbols were seen marked upon each, together with the names of different divinities, to the worship of which, probably, the deceased had been particularly devoted.

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[*] *Lettres écrites d'Egypte et de Nubie*, p. 96.

The papyri exhibited represented Osiris seated in judgment, supported by Isis and Nepthys, the wife and sister goddesses. Before the deity is placed the lotus, and over this the Four Genii of the Amenti*. Thoth, the Egyptian Mercury, is introducing the deceased (a priest apparently, from his habiliments and shaven head), and is prepared to record the judgment on a roll of papyrus, which he holds in one hand, whilst he is furnished with a style to accomplish this in the other. The deceased is in an imploring attitude, with his arms extended towards Osiris, and he is supported by a figure emblematical of Truth and Justice. Above the deceased are ranged, in two rows, the forty-two assessors, as described by Diodorus Siculus; and before the deceased is a balance, in which is being weighed the deeds and actions of his life. His heart is represented in a vase placed in one scale, and a feather, typical of truth, in the other. Anubis (the jackal-headed god, and one of the principal deities of the Amenti, and whose chief office appears to have been to superintend the departure of the soul) is regulating or directing the balance.

Mr. Pettigrew now proceeded to make a few observations on some of the points named by Herodotus, as appertaining to the embalment, and first drew the attention of his auditors to the extraction of the brain through the nostrils. This process, he remarked, would be found to be one of considerable difficulty; and Greenhill, who wrote a work on embalming, treats the extraction of the brain through the nostrils as "an amusing story of a thing impracticable and ridiculous." Mr. P. however, demonstrated the truth of the relation, by exhibiting a specimen of a Græco-Egyptian mummy, in which the process had been adopted in so complete a manner as not to leave even a vestige of any of the membranes surrounding the brain, which usually hold, with the greatest firmness, their attachments to the inside of the skull, and this was effectually accomplished without producing any disfigurement whatever of the external part of the nasal organ. It appears that an instrument must have been introduced up the external nares, made to pierce through the ethmoid-bone, and then by a rotatory motion, Mr. P. thinks, made to break down the surrounding parts of the frontal and sphenoid bones, so as to occasion an opening about the size of a crown-piece, through which the contents of the skull were extracted. Mr. P. has other examples in his possession; but he exhibited the Græco-Egyptian specimen as the most complete. Of the instrument with which this was effected, Mr. P. exhibited a drawing obtained from specimens in the Berlin Museum, and seen in Plate I., (p. 24,) figs. 1, 2. Their composition is of bronze.

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* These are Netsonof or Kebhnsnof, with the hawk's head; Smof, or Smautf, with the jackal's head; Hapee, with the head of the cynocephalus, and Amset with the human head. Amenti or Amunti, in Coptic, exactly corresponds with αϑης in Greek. It signifies both Receiver and Giver; and Mr. Wilkinson has therefore justly remarked, that it implies only a temporary abode. Now it must be recollected, that this corresponds with the idea of the Egyptians returning again to the earth after a short period.

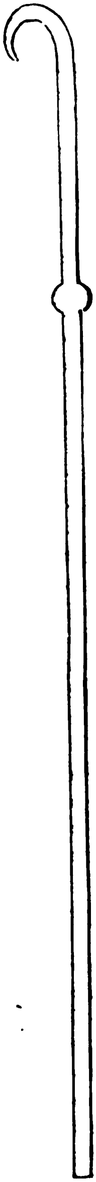
In those mummies in which the second mode of Herodotus was adopted, the brain was not extracted; and Mr. P. exhibited an instance to show that it had settled down into a cake-like mass, and had taken the form of the back part of the skull, being marked by the internal ridges of the crucial spine of the occipital bone, which also clearly showed that the position in which the mummy had been placed, was an horizontal one. After the brain had been extracted from the skull, it was customary to introduce a quantity of linen through the nostrils into the cavity; and in the mummy opened by Mr. Davidson, at the Royal Institution, in 1833, Mr. P. saw in this instance nine yards of linen, three inches in breadth, withdrawn from the head.

In the head of a mummy brought from Thebes by Mr. Wilkinson, in which the brain had been extracted in the manner described, and the nostrils plugged up, Mr. P. found, in the cavity of the skull, a quantity of insects in various stages of maturity, and sufficiently perfect to admit of an accurate description and delineation of their several organs. These Mr. P. has figured in his *History of Egyptian Mummies*, and has named them *Necrobia mumiarum* and *Dermestes pollinctus*. They are species quite distinct from any to be met with in the cabinets of natural history, or in any published work. The Rev. W. F. Hope, the best entomologist of the day, has minutely examined them. He states the *Necrobia mumiarum* to be closely allied to the *Necrobia rufipes* of Fabricius; but he thinks it differs in colour. It is, however, not improbable but that the embalming-materials may have had some effect in producing the present colour of the insects; and Mr. Hope is of opinion that the natural colour of the insect was a violet or deep purple colour. The *Dermestes pollinctus* is also different from any known species of this insect. It resembles in form the *Dermestes domesticus* of Siberia, described by

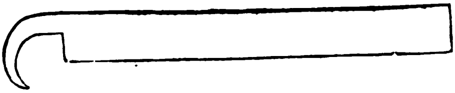
Plate I.

1

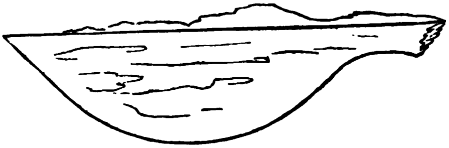
2



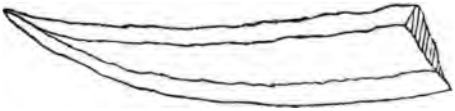
3



4



5



Stevens. Mr. P. obtained from one skull nearly three hundred tolerably perfect insects. There were also abundant fragments; the perfect pupæ were not numerous, but the great quantity of empty cases showed that the greater part had arrived at the imago state some time after the process of embalming was completed. The head was not always filled with linen, for Mr. P. found in another specimen a large quantity of bituminous matter, and the same occurred in an instance to Dr. Mead.

Passing on to the mode of treating the body of the mummy, Mr. P. remarked, that the statement made by Herodotus as to the incision in the *left flank* had been clearly verified, and most mummies presented examples of that part of the process. By this incision the viscera, both of the chest, and belly, and pelvis, were extracted, and the cavities were filled sometimes with the dust of cedar or cassia wood (it is supposed), various aromatics (lumps of myrrh have been found), masses of pitch, and quantities of saline matter mixed with earthy substances. The mummy of Horseisi, unrolled by Mr. P. at the Royal College of Surgeons, in 1834, had a very large quantity of natrum and earth contained within it. The mummy of Kannop, at the London University, was filled with the dust of some wood mixed with aromatics; and the mummy brought into this country by Dr. Perry, and described by him in his *View of the Levant*, &c., which has been unrolled by, and is in the possession of, Mr. P., had large quantities of pitchy matter in all the cavities of the body.

The instrument with which the flank or ventral incision is made, has been stated by Diodorus Siculus to have been done with an Ethiopic stone. Passalacqua found some of pyromachous silex (according to Brogniart), and one of these is figured in Plate I., fig. 5; but in the collection of antiquities belonging to the University of Leyden, there is a knife of silex, which is supposed to have been, from its shape, capable of making the incision at one stroke. It is represented in Plate I., fig. 4.

The intestines, liver, &c., were sometimes returned into the body, each portion or viscus being separately rolled up in bandages. In the mummy of Horseisi there were five distinct portions; these are preserved in the Hunterian Museum belonging to the college. In other instances the viscera have been found placed either upon the body or between the thighs and legs of the mummy; and in Mr. P.'s Græco-Egyptian mummy, he found the heart placed between the thighs, but not enveloped in any bandage. This was the only part from within the body that was preserved in this instance; and it is therefore probable that the other viscera were disposed of in the canopic vases which are so frequently found in the better kind of private tombs, but which did not come into this country with the mummy in question. Mr. P. observed, that it was evident some long cutting-instrument must have been introduced to extract the viscera, for he found in his Græco-Egyptian mummy, in which there was no flank-incision, an opening made close to the rectum, and through which all the pelvic, abdominal, and thoracic viscera had been dragged, that there were at the bottom of the neck in the chest, portions remaining of the wind-pipe, the large blood-vessels, &c., which presented in the most indisputable manner *cut surfaces*. These parts had been preserved in spirits, and were exhibited by Mr. P. on this occasion. The instrument by which Mr. P. conceives the intestines were in ordinary cases dragged out through the incision in the flank, is represented in Plate I. fig. 3, which is a representation taken from a specimen in the fine collection of Egyptian antiquities at Leyden. The edges of the incision in the flank, Mr. P. has never seen sewed together; they have been merely placed in apposition.

According to Herodotus the application of the resinous matter which lies over the surface, and penetrates into the substance of the body of the mummy, takes place after the steeping of the body in the solution of natrum. This must, however, be an error, for it is clear that the salt, which Mr. P. has ascertained to be a fixed alkali composed of carbonate of soda, sulphate of soda, and muriate of soda, and of which large crystals are frequently found in the cavities of the mummified bodies, could not penetrate after the application of the pitchy matter. In this latter process it is clear that a considerable degree of heat must have been employed to promote the absorption of the resinous matter, and this was found in the Græco-Egyptian mummy to be effected in the most perfect manner, for the pitch had, along with aromatics, entered into the most minute cancelli of the bones; the vertebræ of the back were completely saturated with the substance. The epidermis or cuticle of the body is in all cases found to have been very carefully removed, which is most likely done at the time of the steeping in the solution of natrum, and from which previous process the pitchy matter would obtain a much more ready entrance. As the nails are attached to the epidermis, and as the object of the ancient Egyptians appears to have been to preserve, as entire as possible, the body of the individual submitted to this curious process, great care seems to have been taken to prevent their detachment. In several instances in Mr. P.'s possession, the cuticle has been cut off close to the root of the nail, and in some cases, where the whole of the cuticle has been taken away, by perhaps an advanced stage of putrefaction having ensued prior to the embalment, for in a climate of so elevated a temperature as that of Egypt, this decomposition must proceed very rapidly, he has found the nails tied on to the fingers.

Upon the surface of the body it is not unusual to meet with some appearances of crystallization. On the Græco-Egyptian mummy there were so many crystals that Mr. P. was enabled to collect a sufficient quantity to be submitted to the analysis of Mr. Faraday. This talented chemist describes the crystallization as very perfect and acicular, and, from their appearance, he supposes them to be the result of sublimation; but when the substance is heated, it does not prove to be volatile. It fuses, and upon cooling concretes again, crystallizing the whole like spermaceti. It burns with a bright flame, and evidently abounds in carbon and hydrogen. It is not soluble in water, and has the odour when heated, of a fatty matter; but then alkali acts very feebly upon it, and dissolves only a very small portion. On the contrary, it is very soluble in alcohol, the solution being precipitated by water. The substance Mr. Faraday conjectured might probably be a result of slow action upon organic (perchance animal) matter; and has, perhaps, been assisted in its formation by *heat*. These crystals are quite different from those found on the body of the mummy opened at Leeds, which were ascertained to be of natrum employed in the embalming; they rather resemble those described by Dr. Granville, as found studded on the inner surface of the skull of his mummy (*Phil. Trans.*, 1825), and which appeared to him to consist of an animal substance resembling steatine.

The ornamenting of the bodies of the mummies has always excited much attention; but a sufficient number have not yet been examined to determine, with anything like accuracy, the reasons which produced so great a diversity as has been observed. It is probable, as we can now, from a knowledge of the hieroglyphics, ascertain the condition of the embalmed individual, that by an attentive examination and comparison of other mummies, we may be able to ascertain whether particular classes received any particular kind of ornament, or whether it was merely the result of an ability on the part of the relatives of the deceased to go to such an expense in the process. The Græco-Egyptian mummy exhibited by Mr. P. has been gilt over the whole of the anterior surface of the body, from the crown of the head to the extremity of the toes, and large patches of gold, shining with a brilliancy equal to that which it presented on the day of its application are still observable on several places. On the head there remains a considerable quantity, and also on the hands, the thighs, and the legs; a great portion came away with the bandages, which were in this instance fixed with extraordinary firmness by the pitchy matter to the surface of the body. Sometimes the gilding is confined to particular parts of the body, and has been found only on the nails of the fingers and toes, or upon the eye-lids, or the lips, on the face, or on the sexual organ. Abd-Allatif mentions leaves of gold on the forehead, eyes and nose, and on the female organs of generation. He states that it was customary to lay a small leaf of gold on the body, and in some instances to place a lingot of gold in the mouth, and, upon the authority of a cadi of Bousir, three lingots had been removed from the mouth of a mummy, and these weighed nine mithkals, that is one drachm and a half Arabic—ninety grains, one grain Arabic being equal to two-thirds English. A drawing of a plate of gold which, according to the hieroglyphics upon it, belonged to the time of Ramses the Great, was brought from Egypt by Mr. Wilkinson for Mr. Pettigrew, and this is the only instance of the kind known to this distinguished traveller. On Mr. P.'s Græco-Egyptian mummy the gold-leaf appears to have been laid on in square portions, and where the surface has not been completely covered, an additional square piece has been placed, so that the leaf being there thicker, remains more conspicuous than that upon other parts. A large quantity is upon both of the hands, but the nails are not gilt, they appear to have been stained with the henna, the *Lawsonia inermis* of Forskahl, the gopher of Scripture, and the cypress of the Greeks. Mr. P. considers the gilded mummies as belonging chiefly, if not exclusively, to the Græco-Egyptian æra, and he thinks the instances of them are so numerous, that this process is not to be considered as one pursued in persons of the highest rank and importance only, but probably adopted in all those cases in which the relatives were able to sustain the expense charged for such an addition to the ordinary embalming-process. Passalacqua is of opinion, that all the mummies found to have been gilt on the flesh are Greeks,—the mummies of those Greeks who either in the time of the Pharaohs or Lagides were living in Egypt. Many of these have been found with Greek inscriptions or characters impressed on the bandages or cases. The form of the head of Mr. P's Græco-Egyptian mummy would seem to bear out this opinion, for its shape is widely different from that of the ancient Egyptian head, which in its form approaches somewhat to that of the Negro: but the hair, it must be recollected, has never been found to be woolly. There was no inscription upon Mr. P's Græco-Egyptian mummy, but it is not improbable that the outer covering, which might have held it, had been removed before it was brought into this country. It cannot be a matter of surprise that the Greeks, who adopted so much of the mythology of the Egyptians, should also have adopted their manners in the treatment of their dead. The body of Alexander was enclosed in a covering of gold, a sort of chase-work, and of such a nature that it could be applied so closely to the skin, as to preserve not only the form of the body, but also to give the expression of the features of the countenance.

Gold is, however, not the only material found in contact with the body, for various ornaments are frequently met with,—necklaces, composed of different coloured substances, of lapis-lazuli, cornelian, agate, jasper, and basalt. From these Mr. P. has seen suspended a representation of the scarabæus, and those instances have all been priests. The scarabæi are found not within but upon the body, and in contact with the flesh; they have sometimes hieroglyphical inscriptions upon them, either written or carved. Mr. P. found a scarabæus on the mummy of Horseisi, at the Royal College of Surgeons, and also upon Mr. Davidson's mummy. These two mummies were prepared precisely in the same way, the cases were of the same kind and arrangement, the bandages alike, a necklace and scarabæus round the neck and on the breast of each, and the place of eyes supplied with artificial ones, made up of ivory and black composition. They came also out of the same tomb, and were brought into this country by Mr. Henderson in 1820.

The Bandages with which the body is swathed have hitherto been regarded as composed of cotton; but linen has been mentioned by many travellers, though no satisfactory evidence had been offered on the subject until the mummy-cloth from Mr. P's. Græco-Egyptian mummy was examined by Dr. Ure. To this gentleman is unquestionably due the discovery of their proper texture, and this knowledge was acquired by the aid of the microscope*. Dr. Ure and Mr. P. made a precise and accurate examination of the nature of the Egyptian cloth, and the various kinds of cotton; and the following statement may be relied upon as the indisputable result of these researches into the structure of the textile fibres of flax and cotton.

" The filaments of cotton are almost never true cylinders, but are more or less flattened and tortuous, so that when viewed under the microscope they appear in one part like a riband from the one-thousandth to the twelve-hundredth part of an inch broad, and in another, like a sharp edge or narrow line. They have a pearly translucency in the middle space, with a dark narrow border at each side, like a hem. When broken across, the fracture is fibrous or pointed. Mummy-cloth tried by these criteria in the microscope appears to be composed, both in its warp and woof yarns, of flax, and not of cotton. A great variety of the swathing fillets have been examined with an excellent achromatic microscope, and they have all evinced the absence of cotton filaments."

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* It would perhaps be unjust not to mention that, subsequent to Dr. Ure's researches, and Mr. Pettigrew's statement respecting them in his *History of Egyptian Mummies*, Mr. James Thomson published a paper "On the Mummy-cloth of Egypt," in the *London and Edinburgh Philosophical Magazine*, from which it appears that the author had been led to employ the microscope for the same purpose as that adopted by Dr. Ure, and it is satisfactory to find that the conclusions come to by both these experimenters agree in the most complete manner.

Mr. P. exhibited several diagrams to illustrate this part of his subject. In the first place he showed the cotton-fibre penetrated with Canada balsam. This highly-refractive medium makes the edges of the riband-form filament appear thicker, and the middle part thinner than they really are; but it thereby displays more plainly the twisted or tortuous structure of cotton. Another view represented filaments of Smyrna cotton without balsam, viewed in contact with a micrometric glass scale, divided by parallel lines 1000th of an inch apart. The tape-like filaments of this cotton are fit only for making candle-wicks, being very irregular in form and breadth. Another diagram exhibited the fine Sea Island Georgian cotton, with its regular cork-screw fibres, one-half the breadth of the Smyrna cotton, and capable of being spun into threads so fine, that one pound of cotton may reach 210 miles, *i. e.*, from London to Paris in a straight line. Along the middle of the flat filaments of cotton, beautiful veins like embroidery run; and the substance has a pearly lustre in the microscope. The edges have always the appearance of a thickened list. Filaments of flax viewed in air, and also in balsam, were represented. These filaments have a glassy, not a pearly lustre, and they are regular cylinders like capillary glass tubes. When broke across, the fracture is smooth; where the cross rupture of cotton is ragged. When flax is viewed in balsam it appears to have a great many irregular transverse lines, a few of which go round the circumference; but most of them are only partial markings.

These appearances enable us to distinguish flax from cotton with perfect precision, and to detect a single pearly-riband of cotton among a multitude of flaxen cylinders. Not one filament of cotton has hitherto been detected in any of the mummy-cloths unrolled from the Egyptian mummies. The Peruvian mummies, however, of which Lord Colchester brought home specimens from the tombs of Arica in 1831, and presented them to the British Museum, have been found by Dr. Ure's investigation to be swaddled, partly in cotton-cloth, and partly in a mixed fabric woven of cotton carded and spun with the wool of the vicugna, an animal still abounding in Peru. The Peruvians, at the period of Pizarro's conquest of their kingdom, do not appear to have been acquainted with flax.

The quantity of bandages upon some of the mummies is very considerable, as much as 1000 yards have been found. The bandages from Mr. Davidson's mummy weighed 29½ lbs.; those from the mummy of Horseisi, 35½ lbs. The size and texture vary exceedingly; in general they are in the form of rollers, about five, six, or seven inches in breadth and between five and six yards in length. Compresses, generally formed of old linen, oftentimes darned and stitched up, are often placed between the rollers to make the surface even, and occasionally a large sheet or envelope surrounds the entire body. The coarsest kinds Mr. P. has always found placed nearest to the body, the finest towards the exterior; it varies from the finest texture, resembling the most delicate muslin, to that of a very coarse description, almost like sacking. The bandages were not all placed on at the same time, and Mr. P. thinks it would be impossible to apply them with the extreme exactness which is always observed, unless they were put on wet. The different stages of the bandaging is also apparent, from the inscriptions upon different portions; these are always at the ends of the rollers, and generally give the name of the deceased,—it occurred five times in the mummy of HORSEISI.

The colour of the bandages depends upon the matter in which they have been soaked. The Egyptians were well acquainted with the antiseptic quality of vegetable infusions. Mr. Davidson thinks the colour owing to the gum of an acacia,—the sount (the thorn of Thebais of Strabo). Abd-Allatif says, aloes and goudron, others resin. It is probable that various substances have been employed. The colour is fixed; it cannot be washed out. The bandages have been found of different colours, but principally of a chocolate-colour, or a pinkish tinge, or of a nankin, which is the ordinary hue. Mr. P. has seen instances of the union of these in the same mummy, and he has found the same diversity of colour in the bandages enclosing specimens of embalmed cats; sometimes the limbs are separately bandaged. Some mummies have been found without any bandages, they are merely covered with a mat. Belzoni saw two lying quite naked. Mr. P. mentioned a singular instance of a mode adopted in one of the mummies sent over to the British Museum by the late Mr. Salt, which affords evidence of the great care taken to protect the body from being acted upon, either by the atmosphere or by any moisture. The whole of the outer bandages are covered with a coating of varnish of a deep leaden colour, and give to the mummy the appearance of being furnished with a complete coat of armour. The folds of some of the bandages are visible beneath this coating. Mr. P. has not been able to trace any other instance of the kind; it is not noticed by any author he has consulted, or any traveller with whom he has communicated.

Upon another mummy in the British Museum, also sent over by Mr. Salt, Mr. P. found a portrait, the execution of which is well worthy of attention. It would, from its situation and the manner in which it is secured over the face of the mummy, by the bandages, and the entire absence of mythological emblems, appear to be the portrait of the deceased. The colours are very brilliant, and the management of light upon various parts of the countenance corresponds perfectly with that followed by painters of the present day. This mummy would seem to belong to the later Egyptian period, to the time of the Romans, for the dress upon the bust is that of the Roman toga. The whole portrait is of exceeding interest, as one of the most ancient pictures in the world, and as giving to us a faithful resemblance of the people of that country and period. It is painted on a plank of cedar wood; the colours are all vegetable, and fixed by a strong gluten. Mr. P. has given a fac-simile of the portrait in his work on the Egyptian mummies.

The cases in which the mummies are enclosed are of various kinds; the innermost is made of layers of cloth cemented together, plastered within and without, and laced up behind. It must have been soft when laid on, as its shape corresponds to that of the body. The colours of the paintings on these cases are sometimes extremely vivid; the green is the only colour that appears to have faded, and it is sometimes confounded with the blue; the blue is metallic; the yellow, vegetable; the nature of the white has not been discovered; the red is very brilliant. Red, blue, yellow, green, white, and black, are the colours to be found on the cases, or on the walls of the tombs; the drawings are in profile, the Egyptians being ignorant of perspective; their reliefs are notwithstanding full of vigour, life, and expression.

An opinion has erroneously prevailed that the subject of the representation on the cases is a history of the life of the person embalmed within. Sufficient is known of the hieroglyphics not only to question this opinion, but to establish its inaccuracy. They are very similar in most cases, and usually commence with the same symbols. They have been considered as a collection of images offered by the deceased to Osiris; the deceased sometimes taking to himself the name of the god. There can be no doubt, Mr. P. thinks, that an attentive examination of the characters and subjects will satisfactorily convince any one that the subject bears relation to the trial which the soul was to undergo, and the deities, through whose intervention, or by whose intercession it was to pass through the different stages of its progress towards another stage of existence. If a proof were required to show that these inscriptions in general followed a particular formula, it would be afforded by an example Mr. P. had an opportunity of seeing in the collection of Mr. James Burton, where a blank was left for the insertion of the name of the deceased, the other parts being complete.

Some mummies are without any painted case, but placed in a sarcophagus, generally of sycamore wood. Mr. P. possesses one of this kind, which is formed out of the trunk of the tree, the inside of which is scooped out to receive the body, and the surface covered with pitch. Sometimes the painted cartonage is placed in a coffin of cedar, or sycamore, or deal wood; also of the wood of the *Cordia myxa* of Linnæus, of the *Cordia Sebestena* of Forskahl, or of the *Sebestena domestica* of Prosper Alpinus, which are all three of great hardness. These cases are generally marked with the name of the deceased, and the ordinary inscription, and sometimes the figure of an Egyptian deity is to be found within. The human face, carved upon these cases, or formed of composition (with portions of bronze for the eyebrows and eyelids), is often of very excellent workmanship. This coffin very often contains not only the cartonage, but also a wooden one painted, and gives representations of many of the deities belonging to the Egyptian mythology, and this outer coffin is sometimes itself enclosed within a sarcophagus, formed of various materials. In the instance, which was to be examined on this occasion, the outer coffin was placed within a wooden sarcophagus, covered with hieroglyphics. Other sarcophagi are found of rose granite, or marble, or alabaster, or limestone, or Egyptian breccia, or slate, or basalt, or baked earth. They partake either of the human form, or are of an oblong square shape. Specimens of most of these are to be seen in the Gallery of Egyptian Antiquities in the British Museum.

As to the position of the mummies, the only variety consists in the arrangement of the arms; the body is always extended, and the head erect. The legs are invariably placed at their full length, and brought close together. The arms are found either lying along the sides of the body, the palms of the hands in contact with the thighs, or placed upon the groins, or brought forward in contact with each other, or they are placed across the breast, or, as in some rare instances, one arm extended along the side of the body, whilst the other is carried across the chest. Mr. P. believes these postures to have been indiscriminately employed, for they are to be found in males, females, and children. The mummies are all placed horizontally in the tombs and among pits.

The perfection of the embalming may be judged of by the condition of the hair. Belzoni states that he has seen it eighteen inches in length; but Mr. P. exhibited a specimen from Mr. Burton's collection which measured two feet four inches. Mr. P. found on the head of a female mummy, brought from Thebes by Mr. Wilkinson, the hair plaited and turned up over the head in three distinct portions. The manner in which the plait (a triple one) is made, corresponds perfectly with that adopted by the ladies of this country in the present day. Mr. P. produced this specimen to his audience.

The mummies of children, it is very remarkable, are not frequently to be met with—many travellers have made this observation. The smallest, perhaps, of the kind, is in the possession of Mr. P., it must have been an abortion, as it measures only three inches and a quarter. It is enclosed in a wooden case, which is carved to represent the deity Osiris in a sitting posture; the eyes are of enamel, and the whole has been gilt.

By the side, and at the feet of the mummies, are often found the emblems of the trade or profession of the deceased. By the baker, cakes are found; by the artist, palettes and paints and brushes; by the carpenter, a basket of tools, consisting of a saw, mallet, awls, &c. &c. Time would not permit of Mr. P. going into this part of the subject, or even detailing the variety of objects that have been found, many of which were upon the table of the lecturer. Neither could he speak of the embalming of animals by the Egyptians: the quadrupeds, the birds, the fishes, the reptiles, the insects, nay, even the vegetables, said to have been held sacred by these people; a subject replete with interest, and affording matter for the most curious speculation. Nor could he speak of the mummies of the Canary Isles, or Palermo, or Peru, or of the Burman priests, respecting which he has published some most curious information; nor could he allow himself to make any observations on the physical history of the Egyptians and other nations. Previous, however, to the unrolling of the mummy, Mr. P. deemed it advisable to make a few remarks on the hieroglyphical inscriptions, and to describe some of those which adorned the subject about to be developed.

The extraordinary magnitude, and the permanency of the Egyptian monuments, the magnificent temples, dedicated to their gods, and the splendid obelisks, erected in honour of their kings, Mr. P. observed, bespoke a people much advanced in the arts, and indicated a high degree of civilization. The learning of the Egyptians has been made known to us by the sacred historian. By this record we have been taught to believe in the wisdom of this ancient people, and to feel astonishment at the nature of the institutions, the extent of the learning, and the perfection of the arts, attained at so early a period. The records upon the monuments of ancient Egypt but a few years since appeared to be involved in impenetrable obscurity. The darkness which surrounded them had in vain been attempted to be dispersed, and it remained for British erudition and British industry to open the path of discovery from which it now seems almost certain the ancient history and literature of Egypt may be brought to light. To decipher the characters impressed upon the manuscripts and the monuments of the ancient dynasties of the Pharaohs and the Ptolemies, after the laborious but fruitless attempts of ages, is, indeed, a result far beyond the expectation of the most sanguine; and although those to whom we are indebted for the first-fruits of this glorious harvest, are, alas! removed from us, it is satisfactory to reflect that there are a few who still pursue the subject with an ardour commensurate to their ability, and it is to be hoped, therefore, that patronage on the part of the public may not be found wanting to carry the whole on to a complete and triumphal issue. The key to the LOST LITERATURE OF ANCIENT EGYPT was found in the trigrammatic stone of Rosetta, and it is highly flattering to our national vanity to know, that after the almost vain and fruitless attempts of MM. de Sacy and Akerblad, who had succeeded only in making some progress towards the identification of some parts of the second inscription, it was left to the erudite sagacity of a professor of the Royal Institution, the late Dr. Young, "to convert to permanent profit, a monument which had before been a useless, though a glorious trophy of British valour." The inscriptions on the stone are—

I. HIEROGLYPHIC, or Sacred.
II. ENCHORIAL, DEMOTIC, or VULGAR.
III. GREEK.

The Hieroglyphical language is of a triple character; it is chiefly PHONETIC or ALPHABETICAL; next FIGURATIVE, and thirdly SYMBOLICAL: the last portion, the most difficult to be ascertained, forms fortunately the least portion of the language.

There are also three kinds of writing, found upon the papyri, frequently met with in mummies. These are, 1. HIEROGLYPHIC; 2. HIERATIC; 3. ENCHORIAL. The former two are confined to sacred subjects, and the latter is the demotic, or vulgar character of the country; but Mr. P. successfully showed that the enchorial was only a more cursive and rapid mode of writing the hieratic character, and that the hieratic was only a descent from the hieroglyphic itself, which may therefore be regarded as the perfect character of the language.

Mr. Pettigrew next proceeded to describe the cases of the mummy about to be unrolled. The one immediately containing the body was of sycamore wood, in the most beautiful state of preservation, highly ornamented in various colours, representing the face of the mummy, of a deep-red colour, with a carved figure of a beard passing down from the chin. The eyes were of ivory and composition; and the eyelids and eyebrows of bronze, very ingeniously let into the wood. The inside of the case was whitened, and represented various symbolical figures and characters. The figures were those of Netpé and Osiris. Netpé, mother of the gods, being the wife of Seb or Saturn, and the mother of Osiris; and the name of Seb was mentioned in the hieroglyphics, Osiris, the greatest of the Egyptian deities, whose principal office was to judge the dead, and rule over Amenti, the kingdom where the souls of the good were admitted to eternal felicity.

Around the figures of Netpé and Osiris there were various prayers in hieroglyphics, running thus: "Open the Gate of Heaven; open the World; open the Gate of the Region of the Stars; open the Gate of Amenti; the good Region of Osiri;" the man's name.

On the outside, in front of the case or coffin, was represented, immediately beneath the face, some ornamental figures, composing a kind of breastplate, or necklaces, formed of the leaves of the lotus, persea, &c. Netpé, the mother of the gods, with her wings extended over the body of the mummy, and beneath her the four genii of the Amenti; Netsonof, or Kebhnsnof; Smof, or Smautf; Hapee, and Amset; all holding bandages. Also Anubis, Isis, Horus, Thoth, and Nephthys, also furnished with bandages. Towards the bottom of the coffin were represented two crocodile-headed divinities, in a sitting posture; and the Hawk of Phra, or the Sun, with extended wings. At the lower part of the coffin was represented the deceased on one side, with a human head, as qualified lord of the world, and on the other with that of a Ram, as qualified lord of the heavens. Over the feet was an extended figure of the goddess Isis, encircling, as it were, the feet of the mummy, and a line of hieroglyphics, which reads thus: "This is of Isis, who embraces your feet." The Coptic word for foot is R A T, and the hieroglyphics for the same correspond in every letter, and as it is placed in the plural, following the phonetic hieroglyphics for foot, we have the representation of two feet, as the determinative signs of the thing to be represented.

On the back part of the case was a representation of what has been ordinarily described as a nilometer; but which really means stability, and Mr. Wilkinson thinks intellect. It is an emblem referring to Osiris, as the intellectual creator, and thence applied to the intellect of man. It is worthy of remark, that this emblem is distinguished by four bars, or lines, and that the number 4 corresponds to intellect, as that of the number 3 does to matter, according to the ancient philosophers.

Under the feet of the mummy there was a representation of the sacred bull, with the mummy placed on his back; the bull was galloping off as fast as possible, being probably intended to denote the transit of the deceased to Elysium.

The coffin just described was enclosed within another made of deal. Two lines of hieroglyphics ran down the centre to the extremity of the feet, and contained a dedication to "Osiris, the Great God, Lord of Abydus." Also dedications to Ra or Phra, Atmoo, Ptha Sochar, Anubis,

&c. On the inside was a figure of OSIRIS, with a hawk's head and hieroglyphics, giving the name of Simauf. The face of the mummy was carved on this outer coffin, and had eyes made of ivory and composition. Its execution was very delicate and beautiful. These cases, or coffins, were placed within a large sarcophagus, of an oblong-square shape, furnished with pillars at each extremity on either side; in these there were holes, and it is probable, Mr. P. said, that figures of the four genii of the Amenti had been placed upon them.

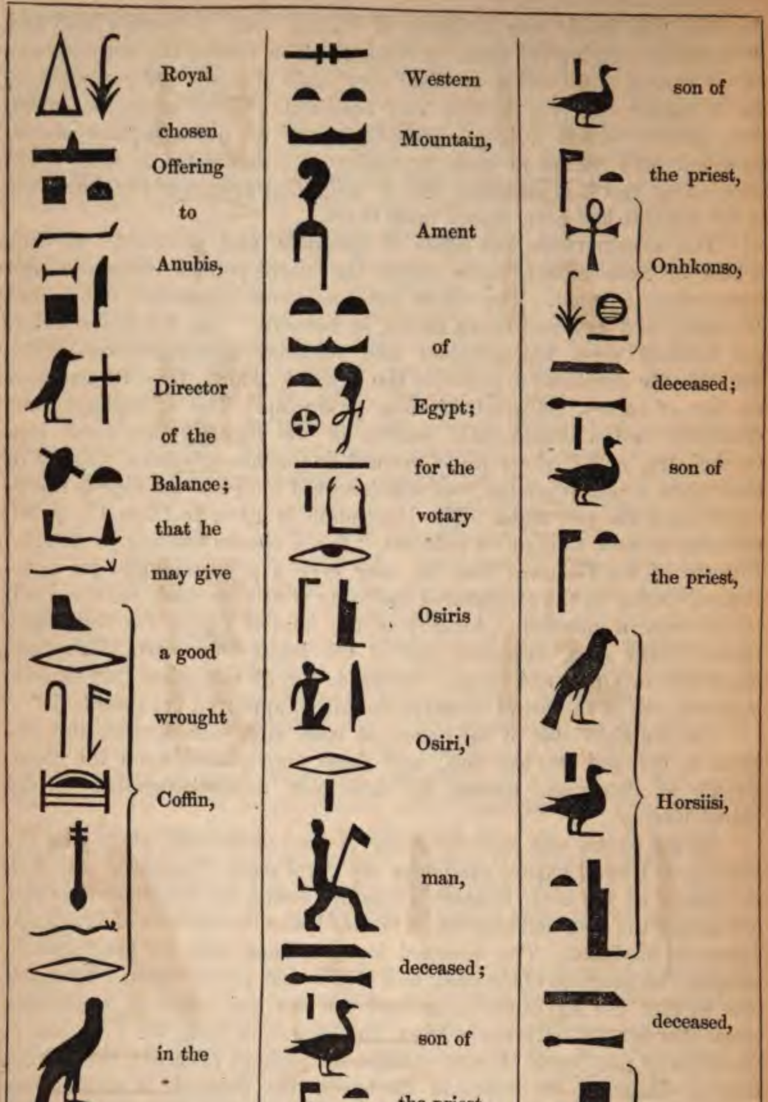
The SARCOPHAGUS was made of sycamore, and plastered over with a kind of lime-cement, upon which the hieroglyphical characters and figures were painted. The whole case measured seven feet two inches in length, and two feet seven inches in breadth. The lid was circular, and covered with hieroglyphics and pictorial representations. The lecturer only exhibited a model of the original, which, from its size, and the fear of injury, he withheld from producing. The sarcophagus was described, and a considerable portion of the hieroglyphics made out. On the case, or lid, there was a vertical line of hieroglyphics, painted in black upon a yellow ground, and which served to divide the objects represented upon the two sides. The inscription is given in Plate II., p. 36, and may be said to read as follows: "Royal chosen offering to ANUBIS, Director of the Balance; that he may give a good wrought (or ornamented) coffin, in the consecrated enclosure (burial ground) for the dead, in the western mountain (AMENT) of the land of Egypt, for the votary OSIRIS, OSIRI man, deceased, son of the priest OUONOFRI, (Onuphris, Manifester or Opener of Good,) deceased, son of the priest ONHKHONSO, deceased, son of the priest HORSHSI deceased, approved (or glorified)."

On the right side of the cover, at each end, were hawks, the emblems of the god RA, the sun; and these were placed upon the hieroglyphic of dominion, having by their side a representation of the Sacred Eye.

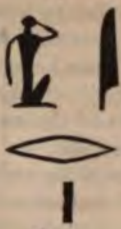
In the centre was seen the BARI, or boat of the sun, conveying the god Ra, or Phra (whence originates the royal name Pharaoh), seated in the centre of the boat, beneath a canopy formed by the serpent Uræus, and having the disk and serpent on his head, and the emblem of Truth and Justice in his hand. The deceased is represented with his head shaved, assisting the progress of the boat, and HORUS acts as steersman. Plutarch tells us that the Egyptians conceived the sun and moon to make their course through the universe in boats; and HORUS, or HOR, Mr. P. thinks, is the origin of the Greek HARON or CHARON, perhaps HAR-ÔNI, the LIVING HORUS. Between the boat and the hawks the deceased is seen on one side, worshipping Osiris and Hapee, and on the other, Osiris and Kebhnsnof.

The left side of the lid has also representations of the hawks, as emblems of the god Ra: and the deceased is seen worshipping Osiris and Amset. The centre of the side gives a very interesting representation, which is presumed to refer to the two natures of man, his *corporeal* and his *spiritual*. A large figure of a man, painted blue, with his arms and legs extended, is made to encircle the painting; and this figure is typical of the heavens. Within it are two figures, seated as judges, placed between heaven and earth: these figures are furnished with the head of

PLATE II.

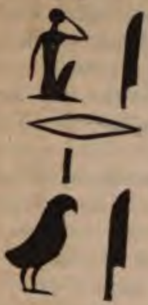

Royal
chosen
Offering
to
Anubis,
Director
of the
Balance;
that he
may give
a good
wrought
Coffin,
in the
Consecrated
Enclosure,
in the
Western
Mountain,
Ament
of
Egypt;
for the
votary
Osiris
Osiri,'
man,
deceased;
son of
the priest,
Ouonofri,
deceased;
son of
the priest,
Onhkonso,
deceased;
son of
the priest,
Horsiisi,
deceased,
approved
or glorified.

1



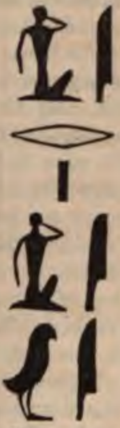
Osiri

2



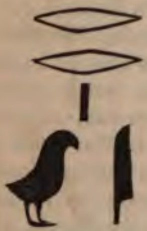
Osiriao.

3



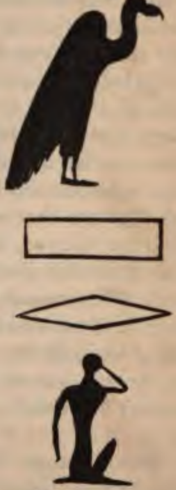
Osiriosao.

4



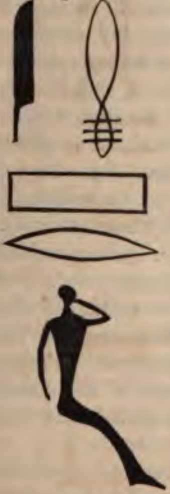
Rereao.

5



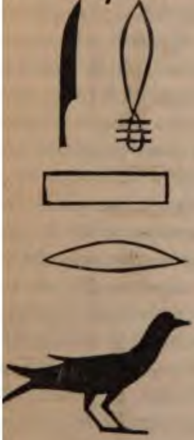
Masharsi.

6



Tasharsi.

7



Tasharge.

a ram, and hold in their hands the emblem of truth and justice. In the centre is a human figure in an erect and firm position, painted blue, with the arms extended up towards the heavens; and falling, as it were, from this figure, is that of a human being, painted red, and is typical of the earthy part of man being thrown off from his spiritual. At the ends of the lid and body of the sarcophagus, are hawks, the Agathodæmon, representations of the sacred eye of Osiris, and a libation offered to the deceased and to Isis.

The sides of the sarcophagus give each seven representations of the deceased; and between these are various lines of hieroglyphics, many of which Mr. P. deciphered. From them we read, "OSIRIS, Hieraphoros of Amun; OSIRI deceased, son of the priest of Amun; OUONNOPHRI (Onuphris of Plutarch, manifester of good, a name of Osiris), son of the priest ONHKOSO, deceased, son of the priest HORSIISI, deceased; his mother, lady of the house, MASHR-SE, &c."

There are some curious variations in the mode of spelling the name of the deceased, and also of his mother, in the hieroglyphics on the sarcophagus, which are highly deserving of notice. His name is spelt in four different ways: 1. OSIRI; 2. OSIRIAO; 3. OSIRIOSAO; 4. REREAO. The name occurs very frequently; but the modes in which it is most generally spelt are the 1st. or 2nd., Osiri or Osiriao. Certain variations often occur in writing the names of Egyptians, which generally consist of the introduction or omission of certain vowels, which in Eastern languages is not of the same importance as in European. In Arabic, Persian, and Turkish, the principal vowel of a word is alone introduced in writing, the force of the others being known by certain marks or points, which are generally omitted except in the Koran. In this case, however, in the fourth example the variation seems to occur in the consonants, which offers some difficulty, since the force of the sitting figure with its hand to its mouth, has been fixed from the word Kaiseros, where it occurs as an S, in more than one instance; its *phonetic* value is therefore to be regarded as determined. Were it not so, one might suppose from the first hieroglyphic in the 4th example in Plate III. (p. 37,) being substituted for it, that this figure merely served to represent a man, and therefore stood for the letter R, the initial of the word Romi (Man); but it signifies a child, as is proved by the position of the hand to the mouth; and the reason of its having the force of S, is that Se, or Sheri, signifies child. Upon the whole, however, the name Osiri is so much like one that we well know (Osiris), and the instances are so many of Osiri or Osiriao, that Mr. P. was inclined to suppose the introduction of the R in this single instance might have been an oversight, or accidental. The name of the mother of Osiri is spelt in three different ways: 1. MASHARSI; 2. MASHARSI or TASHARSI, which is the general mode of writing it; 3. TASHARGE. Perhaps, therefore, the T of Tmau is to be understood, instead of the Tm or m of Tmau, or Maut, (the vulture;) but this is very unusual, the vulture being usually M.

Having thus far remarked upon the process of embalming, the modes in which the embalmed body was afterwards preserved, the character of the cases, and the hieroglyphics depicted upon them; and having shown, that according to these the mummy to be examined was that of Osiri, the son of Ouonofri, who was the son of Onkhonso, who was the son of Horsiisi, and that all these were priests belonging to the Temple of Ammon; that his mother's name was Masharsi, and from the inscription at the feet, and the representation of Isis, that he might probably have been one of the priests officiating in the mysteries of the worship of Isis; it now remained to unfold the mummy and see how far the appearances would correspond with what had been described, and Mr. P. here took occasion to remark that it might possibly occur where a process was so general as that of embalming among the Egyptians, that the body of one person might be accidentally placed in a case belonging to, or intended for, another. In the event of such a casualty, the only proof which could be offered of the identity of the contents of the case was to be found in the name of the deceased being written upon the bandages, as had already occurred in the instance of that of Horsiisi, unrolled in the Royal College of Surgeons. With this preliminary caution, Mr. P. removed the mummy from its case, or inner coffin, and commenced the unfolding. The outer bandages were all marked by the impression of a coating, or dress of beads, made of vitrified earth, of various colours, and which had been separated from their connexions by the destruction of the string upon which they were hung. Several portions were preserved and exhibited. The outer folds of bandage consisted of a large sheet, tinged of a reddish-pink colour, and this was fastened on by some strips of a nankin colour, which were made to act as cords or bands to fix the whole. Beneath the outer sheet the bandages appeared as fresh as the day they were applied, and were passed round the body with the greatest neatness and precision; they were fringed at one end, and the other had a selvedge. They were very numerous, and consisted of scarcely less than 900 yards. They were chiefly 5½ yards in length, and about 6 inches in breadth. They had been formed from cloth measuring one yard and ⅝ in width. The bandages took various directions, and several compresses were found between them. A large portion, resembling a nightcap, was taken off the top of the head. The texture of the bandages was pretty uniform throughout, neither very fine nor coarse. Upon the end of three portions, hieroglyphical inscriptions were found, which seemed to identify the body with the individual mentioned upon the case, and thus rendered the examination very satisfactory. The inscriptions were to the following effect: "life, intellect, power." "Osriis," merely the name, and wrongly spelt. "Linen made bandage eternal (sepulchral) of Osriis."

As the unfolding proceeded, it became evident that the limbs were separately bandaged, and that the bandages near to the body were firmly fixed to it by the pitchy matter that had been used in the embalming. The process of developement, therefore, became necessarily slow; but in a short time one-half of the head was exposed, and was found to be devoid of hair; in this respect corroborating the opinion as to the condition of the priesthood. Near to the surface of the body, a large mass was discovered, which proved to be the liver of the deceased, that had been removed from the body, embalmed, and then placed upon the body, amidst the bandages. Various other portions of the different viscera were afterwards found, and in the most entire state of preservation. These had been extracted from the body through the incision into the *left flank*, as described by Herodotus, and the inside of the body was filled with cedar-dust and aromatics. The ears were remarkably preserved, and their form rendered entire by small dossils of linen being placed into the cavity of the outer ear, and the brain had been extracted through the nostrils, which were somewhat disfigured by the operation. No papyrus was found either between the legs or on the inside of the arms, as is sometimes the case; nor were there any amulets, scarabæi, &c. to be seen. The mummy of Osiri is to be regarded valuable, inasmuch as it affords another proof of the certainty of hieroglyphical literature—his character was found to correspond, as far as possible, with the description upon the case.

It is to be hoped that the trustees of the British Museum will relax from their determination not to allow any of the specimens contained in that national collection to be unrolled, as much curious if not useful information may be obtained by such a research. The Museum ought to possess specimens of mummies in every state and condition, and they should be exhibited in the Gallery of Egyptian Antiquities, in the first, second, third, and fourth series of bandages arranged also according to their condition in life; and their history, as depicted upon the cases containing them, detailed. In this way, the collection, for which lately the Government have so liberally supplied the means for the purchase of Mr. Sams's collection, and various articles from Mr. Salt's, will be rendered truly useful.

A POPULAR COURSE OF CHEMISTRY.

No. IV.

Chemical Affinity.

The elements and their compounds are the materials with which we have to experiment; some of them are very powerful and dangerous substances, and Sir Humphry Davy has well compared these to the refractory spirits of Arabian romance, which, although occasionally subject to the skill of the magician, would often suddenly escape from control, and endanger his person.

All experiments should be undertaken with some degree of caution, for chemical action is frequently amazingly sudden and vehement; in spite of every precaution, the most experienced chemists sometimes meet with accidents, and they are almost certain to happen to the juvenile operator. If he mixes substances together rashly and incautiously, an unexpected result takes place, perhaps a torrent of fumes, or a blaze of flame, is evolved; he loses his presence of mind, becomes alarmed at the spirit which he has thus unwittingly conjured into existence, and, being ignorant of the means of subduing it, he is half suffocated, scalded, scorched, or otherwise injured. Many accidents of this kind have happened in consequence of the ignorance and awkwardness of the young operator, and Chemistry is often denounced as a dangerous and disagreeable study; with a very little care, however, experiments may be performed without injury to himself, or annoyance to others.

Experiments should always be made in a room exclusively devoted to their performance, divested of any valuable furniture, yet well stored with chemical preparations and apparatus, so as to constitute a laboratory, within whose charmed precincts, the young chemist will find hours and days rapidly and imperceptibly glide away; for the "beginning of chemistry is pleasure, its progress knowledge, its objects truth and utility."

Most of the preparations which are requisite for the pursuit of this fascinating science, may now be readily procured at the shops of those who designate themselves "operative chemists," and thus great facilities for the study of chemistry are afforded to the juvenile student. The scene is vastly changed since the time of the alchymists, who were obliged to prepare all their own compounds, or even within the last thirty years, when there was much difficulty in purchasing "chemicals." In those days the possession of any larger quantity than a few drops or grains, of some substances, was only to be acquired by long and anxious labour over the furnace, crucible, or distillatory apparatus; and experimenters had to sacrifice much time in preparing the materials for their experiments. There were but very few shops where chemicals were sold, and that at a most exorbitant rate; *oil of vitriol*, for example, being charged at the rate of thirteen shillings per pound, to obtain which quantity the maker had to work for upwards of seventy hours. The process being gradually improved and expedited, the result became more copious, and it fell to half-a-crown per pound, but even then the supply was limited. At the present day, about fifty thousand tons of oil of vitriol are annually manufactured, and it is sold at the rate of five farthings per pound. *Spirit of salt*, or *muriatic acid*, was formerly sold at a much higher price than *oil of vitriol*, and could only be very sparingly procured; but at present it is so abundantly produced in certain processes of manufacturing chemistry, that it is of less value than that of the glass bottles necessary to contain it; therefore torrents of muriatic acid are daily allowed to run to waste, and it is a matter of no small difficulty with the manufacturer to find out a place where to throw it away. A few years ago *carbonate of soda* was a valuable preparation, and sold for twelve shillings per pound; it may now be bought, dry and pure, for ninepence, or, in the crystalline state, for twopence per pound.

About thirty years ago a single grain of the metal *potassium* was a wonder; there were but few chemists who could elicit it from its combination with oxygen, and it could not be purchased at any rate, therefore its marvellous properties were almost exclusively exhibited in the laboratory or lecture-room of the noble Institution in which its discovery was made. As analytical chemistry advanced, the facilities for procuring potassium somewhat increased, and it could be sparingly purchased at the rate of about five shillings per grain, although many persons refused to part with it even at that price. It gradually became more abundant, and its price lowered in proportion, until at the present day it is kept in the shops as a common preparation, and sold for about fifteen shillings per *ounce;* a *pound* of potassium is now no uncommon quantity.

Although it has been stated that the room destined for the laboratory should be well stored with chemical preparations and apparatus, yet no extensive or extravagant outlay of money in their purchase is meant; on the contrary, a multitude of the most beautiful and instructive experiments can be made, with very limited means, and without any costly showy apparatus. It is better for the juvenile student to obtain a few "chemicals" as he requires them, rather than to fill his bottles and shelves at once with a host of articles selected from a catalogue. Those who are desirous of knowing how a regular laboratory should be fitted up, will find ample directions in Faraday's excellent work on *Chemical Manipulation*, which is also replete with valuable instructions concerning the art of making experiments.

To perform experiments with neatness, safety, and success, requires long and laborious study. A beginner must not expect anything like great precision in the results of his first attempts; nor must he be discouraged by repeated failures, but endeavour assiduously to ascertain the cause of them, and thus he will gradually, yet securely, acquire much valuable practical information. Chemistry is a science of experiment; facts are the data from which conclusions are to be drawn, for no reasoning, *à priori*, can enable us to judge as to the result of any chemical operation.

But now, to commence the more immediate subject of the present paper; and this must be done by referring for a moment to Mechanical Attraction.

Mechanical Attraction principally exists between bodies of similar natures; or, where it takes place between solids and fluids, the bodies undergo no change of their respective natures; thus two pieces of lead by strong pressure unite with each other, and form one mass, which has precisely the same characters and habitudes of either piece of metal separately. If a piece of lead be dipped into water, oil, or spirits of wine, and withdrawn, a drop of any of these fluids will be attracted by the lead, but they are not changed in their characters by this proceeding, for the attraction is merely mechanical, and mechanical means will instantly destroy it; thus the capillary attraction of a cloth or piece of blotting paper, will instantly draw any of these fluids from the surface of the solid lead.

Melt some lead in a ladle, and lay a little lump of tin upon its surface, it will float there, because it is much lighter than the lead; it will soon melt, and when this happens remove the ladle from the fire and let the metals cool. It will be found that, although the tin was the lightest metal of the two, it is not discoverable as a distinct stratum at the surface; that, cut the lump of mixed metals where you will, it presents an uniformity of composition, and nowhere can the tin and the lead be separately seen.

Take an ale-glass nearly full of water, and cautiously pour upon its surface some spirits of wine, coloured red with cochineal; this will float upon the water as a distinct stratum, (which the colour renders very evident, and it is employed for no other purpose,) for spirits of wine is lighter than water. Cover the glass with a card, or saucer, so as to prevent evaporation; take another ale-glass, about one-quarter full of water coloured with cochineal, and fill it up cautiously with colourless spirits of wine, which is best done by letting it run from the pipe of a funnel over which a bit of muslin is tied, this pressed against the sides of the glass, and nearly touching the water, will enable you to pour on the spirit without disturbing the water. Cover up this glass, as well as the former, and leave them for a day or two. At first the liquids preserve their respective situations, in virtue of their different relative weights; but, in the course of time, the lighter spirit will be drawn down through the heavier water, and the heavier water will rise through the lighter spirit, as will be evident from the red colour being diffused throughout the whole contents of both glasses, and when the action has arrived at its maximum, no repose will cause the spirit and water to separate into two distinct strata.

Now these are cases exactly analogous to the experiment with lead and tin, which has been already mentioned, some power of Attraction, very different from mere Mechanical Attraction, is here operating, to cause lighter substances to descend through heavier, and *vice versâ.* It is *Chemical Attraction*,—the lead has a chemical attraction for the tin, the water has a chemical attraction for the spirits of wine; the substances, therefore, although of opposite weights and properties, are enabled mutually to penetrate and combine with each other, in opposition to the laws of gravity, and, when thus chemically combined, they will not separate, by any mechanical means. Every part of the lead, upon analysis, is found to contain tin, and the strength of the diluted spirit is found to be the same from whatever part of the glass it is taken for examination. Chemical Attraction is often called *Heterogeneous Attraction*, its distinguishing feature being, that it takes place between the particles of dissimilar bodies, causing their union, and producing a new and distinct class of compounds; when substances thus attract each other, they are said to possess mutual *affinity*,—hence chemical attraction is far more commonly called *Chemical Affinity*.

To take another instance of this power of attraction. Let us refer to the metals gold and mercury; the one a *solid* metal of a fine yellow colour, the other a *liquid* metal of a silvery whiteness. Bring them into contact, they attract each other with a very considerable force; and that this is not merely mechanical, but chemical, is soon proved by the change of physical characters which the metals undergo by being left for a short time in contact; the gold loses its solidity and colour, and the mercury its liquid form,—the resulting compound is a soft, unctuous mass, of the colour of mercury. That form of gold-leaf called "dentists' gold" is convenient for this experiment, it is very much thicker than common gold-leaf, and therefore more tangible; a little disc of it, about half an inch in diameter, presented to a globule of mercury about the size of this letter (O) will present the result; or several leaves of ordinary gold-leaf may be rubbed with a similar globule in the palm of the hand, with the point of the finger, and the desired compound is produced. Such compound is called an *amalgam**, and no mechanical means enable us to separate the metals after they are thus once combined; but *heat* will destroy the chemical attraction existing between the two metals; the mercury, being volatile, flies away in fumes, leaving the fixed gold in its metallic state. The amalgam, when rubbed upon the surface of a clean plate of copper, adheres to it, and presents a silvery-looking surface, but no gold will appear: expose the amalgamated plate to heat, the mercury volatilizes, and leaves the gold in firm and close contact with the copper; and upon this principle depends the art of *water-gilding*†. Gilt metal buttons are an example of it, they are made of discs of clean copper, to which the amalgam of gold is applied, then heated to expel the mercury, and the noble metal is left in the state called "dead gold," which may be polished to the full extent of its splendour, by rubbing it forcibly with a smooth hard steel, or agate, tool, called a *burnisher*‡.

Silver has a strong attraction for mercury, and forms an amalgam which is not distinguishable in appearance from that of gold, but yielding up silver by heat, and therefore applied for *silvering* the surface of copper. In experiments with mercury it often happens that coin and plate become accidentally spotted with it, thus a sovereign instantly becomes *white*, and no longer passable; heat it carefully in the flame of a spirit-lamp, the mercury volatilizes, and the coin will assume its proper lustre by a little friction. When mercury is accidentally spilt, persons often endeavour to collect the scattered globules in a silver table-spoon, which becomes of course totally spoiled by uniting with the mercury; but heat will volatilize it as in the case just mentioned, and the spoon may then be burnished until it acquires its usual lustre; if, however, the metals have been for some time in contact, the solid texture of the silver is destroyed throughout its whole mass, and then all that can be done is to heat the amalgam in an iron ladle, and preserve the silver for some other experiment. When mercury is accidentally spilt, the globules may be collected in a wooden, or horn spoon, or upon a piece of bent card; rings, watches, and trinkets, should always be laid aside during all chemical experiments, for not only will mercury spoil them, but various acids and gases are sad enemies to ornamental metal-work. This chemical attraction of mercury for the nobler metals is however of much practical utility, as has just been shown when speaking of "water-gilding," also in several metallurgical operations; the ores of silver are reduced to powder, and agitated with mercury for a considerable time, an amalgam of silver is formed, and this exposed to heat leaves the fixed metal in a state of considerable purity.

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* The term *amalgam* is used to denote the compound of *mercury* with other metals; but when they unite with each other to the exclusion of mercury, as in the case of lead and tin, the term *alloy* is applied to the compound.

† Mercury was called *hydrargyrum* by the alchymists, the term signifying *water of silver*; perhaps the term *water-gilding* originated from the circumstance of this "solutive water" being employed to dissolve gold.

‡ The highly-ornamental buttons worn on full-dress coats, furnish examples of gold in its *dead* and *burnished* state, the former is the appearance as it comes from the fire, the latter the result of friction.

Tin and mercury also form an amalgam of nearly the same colour and texture as the two former; it has very important uses, for all our mirrors and looking-glasses owe their lustre to it. A thin sheet of tin, called *tin-foil*, is laid upon a smooth solid table, and amalgamated with mercury, thus presenting a brilliant metallic surface; upon this a perfectly clean and dry plate of glass is slid gently and carefully; equable pressure is then applied, and the glass forcibly adheres to the amalgam in virtue of attraction of cohesion. The superfluous mercury is afterwards drained away, and none is left excepting in actual combination with the tin; this hardens by time into a crystalline texture, as may be seen upon inspecting the back of a looking-glass.

Now it matters not how large or solid the pieces of gold, silver, or tin may be, they will all combine with mercury, and lose their states of aggregation in time; but it will be remarked, that if they are employed in the form of fine leaves, their union with the mercury is vastly expedited: we therefore come to the conclusion that attraction of cohesion influences chemical action. This fact is remarkably exemplified with regard to platinum: in the compact solid state, mercury has no action upon it whatever, and it might be hastily concluded that the two metals have no attraction for each other; but if the mechanical aggregation of the platinum be destroyed, so as to reduce it into very minute particles, mercury will then unite to it and form an amalgam*. Chemists are well aware of such facts, and therefore almost invariably destroy the attraction of cohesion of the substances which they submit to experiment; hence the variety of mortars, mills, shears, and files, with which a well-fitted laboratory abounds. Heat and solution are also resorted to when mechanical means cannot be conveniently used. There are numberless familiar examples of cohesion opposing chemical action; thus the fire-grate does not burn away, because of the strong cohesive force of the iron resisting the degree of heat to which it is ordinarily exposed; nor does a lump of coal suddenly and bodily start into combustion, because it opposes cohesion to the chemical attraction of the oxygen of the air. Reduce an iron bar into fine filings, and a lump of coal into fine powder, aggregation is thus to a great extent destroyed: sift them into a fire, the iron will burn with brilliant sparks, like the firework called "a gerbe;" the coal will suddenly burn with a very luminous flame. A lump of rosin held in the flame of a candle will not take fire; destroy its aggregation by powdering, and then dust it through a flame, and it produces an enormous blaze. The sudden combustion and flash of powdered rosin is often used at the theatres for producing what is called "artificial lightning." The dry vegetable powder called *Lycopodium* is employed for a similar purpose, and does not evolve so much smoke. In all these instances the substances are enabled rapidly to attract oxygen and burn, because their aggregation is overcome by mechanical means.

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* The finely-divided state of the metal required, is known as "spongy platinum," which is obtained by heating its *ammonia muriate* to redness.

Again, a lump of rock-salt, alum, or sugar-candy, thrown into water, will be some time in dissolving, because the attraction of crystallization opposes chemical solution; reduce them to fine powder, and they all rapidly dissolve.

In some instances, even when the aggregation of bodies is destroyed, they refuse to exert any chemical attraction for each other, until a third agent is added to them. Thus, *ink-powders* consist of gall-nuts, and sulphate of iron; perfectly dry and in fine powder, they exert no attraction for each other, but remain a mere mixture, of a brownish colour: add water to this, it overcomes the aggregation of the powder by dissolving the sulphate of iron, which exerting its chemical attraction for the matter of the gall-nuts, unites with it to form a new and distinct compound, of a black hue, viz. writing-ink. *Soda-water powders*, or *saline powders*, are instances of the same kind: they consist of *tartaric acid* and *carbonate of soda*, both perfectly dry, powdered and *mixed* together, and in this state they will remain for years without showing any tendency to combine; their respective particles are not endowed with freedom of motion so as to come into close contact: add water, it overcomes their remaining cohesion, they dissolve, attract each other chemically, producing a solution, in which, if proper proportions are employed, the taste of neither substance is perceptible, and this union is attended with the escape of a vast quantity of gaseous matter, forming the well-known effervescence.

If sand, carbonate of soda, and red-lead be reduced to fine powder, and intimately *mixed* together, they show no tendency to combine. If water is added, the carbonate of soda dissolves; but no other result takes place, excepting that the sand and red-lead sink as an insoluble mixture to the bottom of the vessel containing the experiment. We must seek then another agent to cause the union of these three distinct and opposite bodies: expose the powder to a very strong heat, the cohesion of its constituent particles will be overcome; they melt, and in this fluid state begin to exert a strong chemical attraction for each other, and ultimately produce a compound which is solid, hard, transparent, and brittle, namely *glass*.

The whole art of making this truly wonderful and important substance depends upon Chemical Attraction; it is another instance of the application of science to purposes of practical utility. There are *few compounds in which* the properties of the components are more *completely disguised* than in glass. Who would imagine that glass, transparent and beautiful as it is, consists of three opaque bodies of such distinct and opposite natures? Water poured into a glass will no longer be able to dissolve away the soda, as it did from the mixture before fusion; and this is a remarkable instance of aggregation preventing chemical action, for if the glass be reduced to a moderately-fine powder, water will then instantly dissolve out the soluble soda*.

Soda presents an example of what chemists call an *alkali:* there are several such bodies, and they are all characterized by changing the yellow colour of vegetables to a reddish-brown, the yellow of the *turmeric* particularly. Paper stained with the watery infusion of this vegetable is called *test-paper.* Place a bit of glass upon it, moistened with water, no change of the yellow colour results: it, however, instantly changes to brown if powdered glass be so treated, thus indicating that *alkali* is abstracted by the water when aggregation is to some extent destroyed. *Acids* are opposed to alkalies, and, generally speaking, they change vegetable blue colours to *red:* paper stained with watery infusions of violets or *litmus*, is used as a test-paper for acids. Thus the *tartaric acid* instantly reddens litmus paper; but carbonate of soda, or soda, will restore the blue colour: in other words, it is said to *neutralize* the acid; hence when *soda-water* powders are dissolved in water, the resulting solution is *neutral*, neither acid nor alkaline.

The production of colour, or its modification by chemical agents, is the foundation of the arts of colour-making, dyeing, and calico-printing. Thus, lead exposed to heat absorbs oxygen, forming a red oxide, which is employed as a pigment, under the name of *red-lead.* If a piece of white cloth be immersed in a solution of sulphate of iron, afterwards dried, and then soaked in an infusion of galls, these two substances will attract each other in the fibre of the cloth, and form ink, with which compound the cloth is permanently dyed. If any design, such as a letter or flower, be drawn upon white calico, with a solution of prussiate of potash, which is bright yellow, allowed to dry, and then soaked in the solution of sulphate of iron, the two substances will attract each other, and produce a beautiful blue (prussian blue), wherever the design extends. This is the principle upon which one department of the beautiful art of calico-printing depends. If a strong solution of the well-known chloride of lime be spotted over the black cloth, the dye is destroyed wherever it touches, and white spots appear upon a black ground; and if the dyed or printed cloths be immersed in this solution, they become perfectly bleached, in consequence of the destruction of dye and colour by the agency of *chlorine.* This is the principle of the whole modern art of bleaching.

Substances which are useless, or of little value alone, form, by chemical attraction, compounds of vast utility. The skins of animals in a recent state do not admit of many useful applications; they are gelatinous, and prone to putrefaction and decay. The bark of trees is of little value, excepting for fuel; it is also apt to become mouldy and rotten: but if an infusion of bark be made, and a recent skin thrown into it, and suffered to remain for some weeks, the animal and vegetable matters will be found to have entered into mutual combination, producing a substance, having the *form* of the skin, it is true, but neither its texture or habitudes; unlike its components it is insoluble in water, and, so far from being prone to putrefaction, it is remarkably permanent and of extreme utility, admitting of manifold applications in the arts and manufactures, and known as *leather*, produced by the art of *tanning**. A solution of glue or other animal jelly, added to infusion of oak-bark, will instantly form a solid mass, which is, chemically speaking, *leather*, wanting only *texture*.

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* *A fact discovered by Griffiths, Quarterly Journal*, xx. 262.

Leather, as it comes from the tanners, is of a nutmeg-brown or russet-colour, and was formerly exclusively employed in this state, for harness, sandals, &c. Accident, perhaps, first showed that the contact of iron discoloured it, a sword might have been left in a wet leather scabbard, or a spur upon a wet riding-boot, which would both become black wherever there was contact with the steel or iron; this, probably, gave the first hint of dyeing leather black, and gradually solutions of iron were employed for the purpose, under the name of "Copperas Waters." Copperas is sulphate of iron, and a little of its solution washed over russet leather will instantly dye it black, on account of chemically uniting with the matter of the oak-bark which the leather contains, and forming ink.

All black leather, excepting Japan leather, is dyed with solutions of iron,—generally the sulphate of iron; other colours are produced by different metallic solutions, as will be fully shown, when particularly discussing the chemical arts.

The action of various acids upon the metals presents some very important and pleasing instances of chemical affinity, a great number of compounds are thus produced which are called *salts*, all of which have their uses, either in chemistry, medicine, the arts, or manufactures†.

If a piece of pure silver be placed in a glass containing nitric acid, diluted with three parts of water, (and distilled or pure water is in all cases to be understood‡,) a violent action and a vast evolution of dark orange-coloured vapours takes place; the silver rapidly diminishes in bulk, and ultimately completely dissolves, forming a transparent colourless solution, (if the silver be thrown into the acid in fine filings, the action is still more energetic, and part of the materials fly suddenly out of the vessel and are lost, it is therefore better to employ the metal in its more aggregated state.) Now this is a curious and remarkable phenomenon, consequent upon the attraction which the two substances have for each other; no ordinary solvent will act upon silver, and therefore its solution in this instance is totally unlooked for; it proves the impossibility of anticipating the result of chemical action, by any reasoning *à priori*, it can only be determined by experiment. Add silver to the solution until the acid refuses to dissolve any more, or is *saturated* with the metal, then pour off the clear solution into a cup or glass, set upon hot sand, and allow it to cool very gradually; determinate figures will soon make their appearance throughout the liquid, they will gradually increase in size until a considerable mass of white and beautiful crystals form, which are those of the *nitrate of silver*. Thus you have an example of the union of a liquid acid with a solid metal, producing a solid crystalline body, which is a *salt* of silver. A piece of copper acted upon by the diluted nitric acid, will readily dissolve with the disengagement of the same orange-coloured vapours; but the solution, instead of being colourless, has a beautiful deep *blue* tint, and if saturated and allowed to cool, as just directed, it yields blue crystals of *nitrate of copper*. Dry both crystalline products between two or three folds of blotting-paper, and leave them exposed for a few hours to light and air, the salt of silver will soon become purple, dark purple, and lastly, black, the salt of copper will lose its solid form, and gradually pass into the liquid state, but undergo no change of colour. Place portions of the two salts in a dark room for a similar length of time, the nitrate of silver will remain solid and white, that of copper will still *deliquesce* exactly as before. We find, then, that the presence of light, which is an imponderable element, affects the nitrate of silver; but what causes the change in the other? Nothing more than the watery vapour in the atmosphere, for which it has a strong attraction, and which therefore dissolves it when exposed.

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* Probably the most ancient art on record.

† In making these solutions of metals in acids, and indeed in all cases where fumes are evolved, the vessels containing the experiment should be placed beneath *a chimney, so that the fumes* may be carried away without annoying the operator.

‡ Distilled water may be abundantly obtained from the waste-pipe of a hothouse, or other building heated by steam: the waste steam from any boiler, if condensed in a worm-pipe, will furnish distilled water of purity sufficient for all ordinary purposes. Recent rain-water may be substituted for distilled, if there is much difficulty about the distillation.

The blackening effect of light upon nitrate of silver, is applied to domestic purposes, for marking linen. *Permanent ink* is a strong solution of it, which has a powerful attraction for vegetable fibre; many dyes for the hair, also, contain this salt; ivory, bone, and leather, are also frequently stained black by its employment; if any portion chances to touch the fingers in the above experiments, they will be indelibly dyed black. The crystals of nitrate of copper are highly soluble in spirits of wine; and if the solution be kindled, it burns with a lovely emerald-green flame, hence it is abundantly employed in pyrotechny, which is completely a chemical art; sponges soaked in the alcoholic solution of nitrate of copper, and suspended by fine wires over the stage of theatres, produce the lambent green flames now so common in incantation-scenes; strips of flannel saturated with it, and applied round copper swords, tridents, &c., produce, when lighted, the flaming-swords and fire-forks, brandished by the demons of such scenes: the chief consumption of nitrate of copper is for these purposes. In theatrical displays, the skill of the chemist is everywhere called into activity, producing not only various-coloured flames and magical appearances, but also contributing to the splendour of the scenery, dresses, and decorations, the perfection of the gas-lights, and to the efficiency of the ventilation. Many other amusements depend mainly upon chemistry for their interest and perfection.

The nitrates of silver and of copper will yet further present some curious information. *Take the solution of the first, diluted with about half its weight of water in a glass*, and place in it a slip of bright copper. The instant that this metal comes into contact with the solution a remarkable result takes place; the silver begins to reappear in a crystalline *metallic* state, and the solution assumes a bright *blue* colour; let the experiment continue until silver ceases to deposit or precipitate, then brush it all away from the copper, allow it to subside, pour off the blue solution, add a little water to the residuum, pour this off after further subsidence, into the first portions, and thus wash the silver until the water is no longer blue; then collect, dry, and weigh the silver; it will present the same weight as that of the pieces originally dissolved*. This is a process of great importance in the art of assaying and metallurgy, and is constantly practised by the workers in silver, to recover the precious metal from its solutions, and, as it appears in a solid state from a solution, they call it "water silver." The theory of the operation is simply this, the metal copper has a stronger attraction or affinity for nitric acid than the silver has, and, therefore, when put into the solution of the nitrate, it robs the silver of the nitric acid, and compels it to precipitate in its metallic state; the copper taking its place in the solution to form nitrate of copper. This may be proved by evaporating the blue solution and washings nearly to dryness, then cooling it; for the production of crystals, which will appear in the full enjoyment of all the characters of deliquescence, solubility in alcohol, &c., which were found to belong to the nitrate produced by the direct union of nitric acid with copper. Take either solution of nitrate of copper, diluted with water in a glass, as before directed, and place in it a plate of silver; no action will ensue, because the nitric acid has the strongest attraction for the copper, but remove the silver, and substitute a plate of clean iron, a very different matter ensues: the instant that it touches the blue solution it becomes covered with a brilliant coating of metallic copper; upon continuing the experiment, the solution assumes a *green* colour; and ultimately no more copper is deposited; then decant the solution, separate, wash, and dry the copper, as before directed with the precipitated silver, and, if the nitrate employed was that produced by the direct union of the acid and copper, the weight of the precipitated metal will be that of what was originally dissolved. The theory of this process is analogous to that of the former one; the iron, in this instance, has a stronger affinity for the nitric acid than the copper has, therefore robs it of the acid, compels the copper to precipitate, and takes its place in the solution, forming *nitrate of iron*.

This experiment is not without its practical applications in the arts, for recovering copper from its solutions in various acids; the water which is found in copper-mines very frequently contains a large proportion of soluble salts of copper; this was formerly allowed to run waste. The story goes, that a miner accidentally left his iron pick in the water, and, next day, upon resuming his work with it, he found that it was converted into copper. This led to an examination of the cupreous waters, and chemistry soon divested the matter of the marvellous transmutation, by pointing out that it was a case referrible to affinity, and still further proving that it might be employed to advantage. Accordingly, quantities of refuse iron are now thrown into such waters, and allowed to remain until they precipitate the copper, which, being collected and melted, furnishes a notable proportion of the metal of commerce.

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* The most convenient way of collecting the silver is to get it into a small saucer, and let the water slowly evaporate from it; if it is poured on a filter much of the silver adheres to the paper, and cannot be readily detached.

The green solution of *nitrate of iron* resulting from this last experiment, if evaporated with the view of obtaining crystals, turns of a deep-brown colour, and refuses to show any tendency to crystallize; it is an uncrystallizable deliquescent salt; nor can we, by placing any other metals in its solution, cause the iron to precipitate in the metallic state: it has got the nitric acid, and holds it in defiance of the attacks of other metals; its affinity for the acid is superior to any of them. There is no direct method of obtaining the iron from the nitrate.

All the foregoing experiments were invented by the alchymists, and described by them under very fanciful names. They were also frequently adduced as instances of the real transmutation of copper into silver, iron into copper, &c., and, in the early dawn of science, such a conclusion was by no means absurd or preposterous. "He who first saw the corrosion of a metal by a limpid liquid,—who beheld the opaque and ponderous body gradually disappear, and become part of a transparent and apparently homogeneous fluid, and who saw the same metal reappear upon the addition of a proper precipitant, must have been infinitely surprised, and struck with admiration of the occult powers of nature[*]."

If we subject some of the scarcer metals to the action of nitric acid, the results are very remarkable; potassium may be selected as an instance. Drop a globule of it, about the size of a large pea, into a small cup nearly full of water, containing a drop or two of strong nitric acid; the moment that the metal touches the liquid, it *floats* upon its surface, enveloped with a beautiful rose-coloured flame, and entirely dissolves. This is an example of intense Chemical Attraction, existing between the two substances, and the usual attendants upon it are the evolution of light and heat. Try the solution with a piece of turmeric-paper; if its colour is changed brown, a drop or two more acid must be cautiously applied,—if, on the contrary, it reddens litmus-paper, a small globule or two of potassium is required; the object being to obtain a neutral solution: this is easily effected, and now if it be carefully evaporated to about half its bulk, and set to crystallize, beautiful crystals will begin to form, which are those of the *nitrate of potash;* commonly called *nitre*, or *saltpetre*.

The theory of the experiment is simple, yet highly instructive; water (as will be hereafter fully proved) consists of oxygen and hydrogen. Potassium has an intense attraction for oxygen, it therefore snatches it energetically from combination with hydrogen, with such rapidity and force, as to evolve heat sufficient to cause the hydrogen to burst into flame; the oxide of potassium (potash,) thus produced, is instantly attracted by the nitric acid in the water, forming solution of nitrate of potash, which readily yields crystals of a six-sided form[†].

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[*] Bergman's *Opuscula*.

[†] *In all the foregoing experiments upon the solution of metals in nitric acid, they* become *oxidized* at the expense of part of its oxygen, and the *oxides* thus produced, combine with other portions of the acid to form *nitrates*.

Place some dry crystals of nitrate of potash in a glass retort, provided with a proper receiver, and pour on the crystals some *oil of vitriol*, or *sulphuric acid;* apply a moderate heat, red fumes will soon appear in great abundance, which passing over and becoming condensed in the cold receiver, present pure liquid *nitric acid;* the *sulphuric acid* expelling it from its combination with potash, and therefore sulphate of potash remains in the retort. That this new result is the nitric acid, may be proved by causing it to act upon silver, copper, or iron; their respective nitrates will be as readily produced, as they were by employing the acid of commerce purchased at the chemist's.

After having ascertained this fact, dissolve some lead in diluted nitric acid, and thus make a solution of *nitrate of lead*, for an experiment which will be required immediately. Now examine the sulphate of potash in the retort; very likely it is a little acid from *excess* of sulphuric acid remaining in it; if so, neutralize this,—it may be done with potassium; but this is rather too expensive, therefore employ a solution of pure *potash.* The neutral sulphate of potash is very insoluble in water, and crystallizes in shorter prisms than nitre; but you can obtain a solution of it in about 16 parts of water; add this to the neutral solution of nitrate of lead, and note the result; both solutions were transparent and clear, but upon mixture, a copious mass of *white solid* matter appears, which, upon a short repose, precipitates, leaving the supernatant liquid clear and colourless. This is an example of *double decomposition*, the theory of which may be easily understood. Sulphate of potash consists of sulphuric acid and potash, or oxide of potassium, nitrate of lead consists of nitric acid and oxide of lead; but, when the two salts are mixed together, the sulphuric acid having a stronger affinity for the oxide of lead than the nitric acid has, combines with it, forming sulphate of lead, which being very insoluble, precipitates in the solid form: the potash thus freed from union with sulphuric acid is instantly attracted by the nitric acid, which existed in the nitrate of lead; therefore a nitrate of potash is formed, which remains in solution, and its crystals may be obtained in the usual manner. So that in this experiment there is an interchange of acids between the oxides of the two metals; two soluble salts producing one that is insoluble, and another remaining in solution.

Such then are a few of the manifold results of Chemical Affinity, and a passing notice or so regarding their application to some of the arts. The object of this paper is to put the student in possession of a general notion of chemical operations, without entering particularly into all their minutiæ; the application of the theory of definite proportions to some of these results, and the consideration of others in which chemical action takes place more intensely and suddenly, will form matter for future discussion.

DESCRIPTION OF A RATIONAL LUNARIUM.

What vague and false notions of the planetary system common *orreries*, or *planetaria*, invariably convey to the learner, who receives his first ideas on the science of Astronomy by means of them, must strike every one who is curious enough to examine a beginner as to the progress he has made. The reason is palpable; those who recommend the use of these machines, as capable of facilitating the acquisition of ideas on what they regard as an abstruse subject, decide from the well-known Horatian maxim; but they do not consider, that unless the associations early excited by impressions from visible objects are perfectly consistent with truth, their vividness tends to render nugatory all attempts to correct those erroneous impressions by subsequent study. These advocates forget that the absurd misrepresentations of relative magnitudes and distances, which result from the attempt to explain a great number of celestial phenomena by one machine, make impressions on the mind of ordinary learners which are too powerful to be subsequently effaced by abstract numerical details, or by pure mathematical reasoning.

If the common Orrery were only had recourse to when the mind of the pupil had been habituated to comprehend very abstract ideas, and to control the impressions derived from his senses by the exercise of his judgment, there is no doubt that it might be advantageously made use of on some occasions; but this is not the case with the popular mode of teaching; the Orrery is shown to the learner before he has the slightest correct conceptions on the subjects,—probably before he has received even the most elementary instructions in plane geometry. Who, therefore, can be surprised if the false ideas imparted by the visible machine before him, cannot be counteracted by the teacher's exhortations not to pay attention to the magnitudes and distances of the representative planets.

This evil might perhaps be submitted to, if there were any counterbalancing advantages; but the ideas which ordinary orreries are intended to convey, are precisely those which there can be no difficulty in acquiring from verbal instructions, or by means of good diagrams. The general conception of bodies revolving in space round a central one, at different distances, and with different velocities, is too simple to present any difficulty to the slowest comprehension, and a few concentric circles, at the correct proportional distances, drawn on paper, are quite as adequate to assist the understanding as the most elaborate planetarium, and do not convey any false impressions. The common anxiety of machinists to show their skill by ingenious combinations of wheelwork, induces them to aim at making complex machines, by which the planets are carried round in their orbits; but to effect this, they are obliged to violate still more flagrantly proportional distance, and even then without arriving at anything like the correct motions which require illustration. Added to these elementary motions, orreries are intended to explain the phenomena of night and day, of the seasons, the lunar phases, and eclipses, &c.; to effect all this, the falsifications we allude to are carried to a most ludicrous extent, till the machine becomes only a fertile source of every erroneous notion that can be conceived on the subject.

We must here mention that our objections are only against Orreries or Planetaria, and do not apply in any way to Globes, which are as deserving of eulogium as the former are of ridicule. The astronomical or geographical phenomena which it is the object of a globe to elucidate, are really made more comprehensible by such an auxiliary; and, while affording this help to the learner, the globe actually rectifies the erroneous impressions previously received from his senses, it constantly reminds him that the inequalities on the earth's surface, which are so great in relation to him, and to the minute portion of that surface he can view at one time, are really insensible in relation to the whole mass. The mind conceives the true nature of the "vast unfathomable ocean" in reference to the earth, when it perceives, from a simple calculation, that the thickness of the paper, covering the artificial ball, is a tolerable representative of its real average depth; and how much are the wonders revealed by geology rendered intelligible, when the learner acknowledges that a grain of sand, stuck on his globe, is a correct model of Darwhal Ghiri, or Chimboraço, and a scratch with a pin exaggerates the deepest natural valley, or the slightest puncture the deepest mine which human labour has ever excavated. But the sublime ideas of creative power, which the law of gravitation must excite, when the mind rightly conceives the comparatively minute masses acting on each other at enormous distances, remain undeveloped in that which has imbibed its notions on the subject by means of a two-inch world, stuck on a brass wire at perhaps four inches' distance from a half-inch sun[*].

These remarks have been suggested by a *Lunarium* sent to us by a friend, who, agreeing in our opinions on the worthlessness of common machines, has endeavoured, in that before us, to remove their defects, and to accomplish what they are perfectly incompetent to do. The merits of this rational "toy" are, that it can be made by any one who has a little ingenuity, and that, with this simplicity, it effects, with *accuracy*, all it purposes; it is, in short, the contrivance of a mathematician and philosopher; and we think many of our readers will thank us for such a description of it as will enable them to make and to adjust it.

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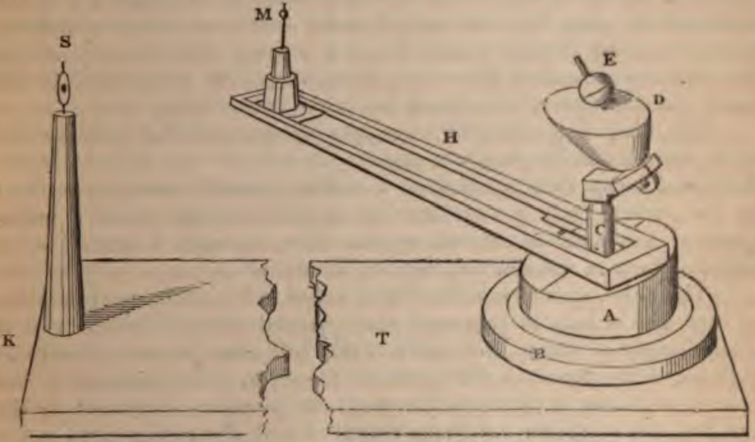
[*] Every beginner should learn, by heart we might say, Sir John Herschel's *receipt* for an Orrery: we give it here with some alterations, for the sake of supporting our views on the subject by such an authority.

Choose any large level field. In the middle, place a globe two feet in diameter; this will represent the *Sun;* place a grain of mustard-seed at 82 feet distance from the sun for *Mercury;* a pea, at the distance of 142 feet from the sun, will represent *Venus.* Our *Earth* will be another pea, at 215 feet from the sun; *Mars* will be a large pin's head, 327 feet off; four grains of sand, at distances of 5 to 600 feet, will represent the new planets. *Jupiter* will be a moderate-sized orange, a quarter of a mile from the sun. *Saturn* a smaller orange, two-fifths of a mile, or *1408 feet, from the sun*; and, lastly, *Uranus a full-sized cherry,* or small plum, three quarters of a mile from the sun. "As to getting correct notions on this subject by those very childish toys, called orreries, it is out of the question." (Sir J. Herschel's *Astronomy;* Lardner's *Cyclopædia,* p. 287.)

This is the smallest scale on which an orrery could be constructed to show the satellites and the smaller planets. The moon, in the above model, would be a common pin's head, six and a half inches from the earth-pea. When we conceive this model, and reflect that the two-feet globe keeps the plum in its orbit at three quarters of a mile distance, and that the two oranges act on the plum, and the peas, and on one another, and still more on the pin-head moon, in all possible positions, as they revolve round the globe, the mind begins to get a glimpse of the power of gravity.

The board which serves as a stand, represents a plane *parallel to the ecliptic.* The cylindrical block A has its upper surface cut obliquely, so that its plane may form an angle of 5° 9′ with the ecliptic; that being the inclination of the moon's orbit to it. This oblique surface is covered with paper, and a graduated circle described on it, through which a diameter must be drawn, correctly parallel to the intersection of the two planes, to represent the line of the moon's nodes; the ends of this diameter must be the 0°, and 180° of the division on the circle. The block A turns round in, and concentric with, the ring B, on the surface of which a graduated circle is also drawn on paper: this lower piece B is made with a pivot to fit into a hole in the board, to admit of A and B being turned round together on their centres. The pillar C is screwed fast into A; it has an oblique shoulder to carry the wire axis of the earth at the proper inclination of 66½° to the ecliptic; this shoulder turns on C to allow of the earth's axis being set to point to the proper sign of the Zodiac in the circle on B.



H is a light frame to carry the moon, M; the opening is contracted to the thickness of C at one end, by two ivory (or brass or wood) slips fixed on its under-side and *flush* with that face; when H, therefore, is turned round, resting on the oblique face of A, these slips prevent any lateral motion. At the other end of H a square plate of ivory (wood or brass) is screwed on the under-side also, the *upper* face of this plate, therefore, coincides with the plane of the surface of A: on this plate a graduated circle is described concentric with the moon's axis. The moon is carried on a double pedestal, the lower piece having a pivot to turn in the central hole of the plate, the under-face of the pedestal is cut bevelled to form an angle of 5° 9′ with its axis, so that by turning the pedestal round, the axis of the moon may be made to stand perpendicular to the *ecliptic* in every position of H, instead of being perpendicular to that frame, as it would be without this contrivance.

The length of the frame, H, *being decided on at pleasure,* (*the one before us is ten inches, but* the larger the better,) divide the length of the opening, from the centre of the circle on the plate, into thirty-two parts, (thirty being the moon's *mean* distance in earth's diameters.) Then, making the centre of the circle zero, mark the 30th division on the ivory slips, and subdivide a division or two on each side into tenths, to serve as a scale for setting the moon at her true distance from the earth. The ball E, to represent the earth, must be made accurately equal to one division in diameter, or one thirtieth of the mean distance, and it must have an equatorial line drawn on it: the moon, M, is to be made ·36 of the earth's diameter, and may be measured from the scale on the slips. In the instrument we are describing, the moon is made in one piece with the upper part of its stand, which turns round on the lower pedestal, before mentioned, to allow of the unblackened half of the surface being turned in the *direction* of the sun, as indicated by the line BK, without altering the adjustment of the pedestal. But if a larger scale be adopted, the moon may be made to slide on a fine needle stuck perpendicularly into the pedestal; in either case there must be contrived means by which the moon may be set correctly at the same height above the surface of the plate, *when the moon is in her node*, that the earth is raised above the centre of the surface of A; or in short, that the line joining the centres of the two balls may, in that position, be parallel to the board, and consequently represent one in the true ecliptic.

The dial, D, is intended to supply the place of meridians on the earth, and should be drawn on paper with care, for the latitude of the place, and being then mounted on a conical piece of wood, cut to have its flat surface truly to represent the horizon of that place: a hemispherical hole must be cut out, so that when the earth is sunk in it the radiating lines of the dial may tend to its centre*.

The pillar, K, is to indicate the *direction* in which the sun is, a piece of card with a hole to represent the *apparent* diameter of the sun is stuck upright on its top, the centre of this hole must be made exactly as high above the board as the centre of the earth. The diameter of the hole is of course to be made the chord of the angle, subtended by the sun from the earth, the arc being described with the radius equal to the distance of K from E.

There must be a counterpoise put on the end of the frame, H, to keep its under-side close down on the top of A, and to steady it while it is moved.

To adjust the Lunarium. Turn B round on its axis till the line AK of the sun's direction is opposite the degree on the graduated circle, corresponding to the sun's longitude for the time. Then turn A round in B, to bring the moon's ascending node into its proper degree on the same circle. Turn the earth's axis round on C till it lies in the plane of the solstices, perpendicular to the ecliptic. Raise or lower the dial and earth till the centre is perpendicularly over that of the pillar C, or over the centre of the ring B: set the dial in the meridian, as indicated by the earth's axis. Turn H round till the line of the nodes on A coincides with that joining the axis of the earth and moon; when in this position, raise or depress the moon on its pedestal till its centre is at the same height as the earth's above the surface of the board; that is, the moon must be put really in the ecliptic when she is in her node; and the corresponding adjustment for the sun's centre may be made at the same time. Then move the frame, H, till its axis, or the line parallel to the *radius vector* of the moon, point to the degree of longitude on A, in which the moon is at the time of adjustment; in this new position, turn the lower pedestal of the moon round on its axis, till that axis is perpendicular to the ecliptic; a small index stuck in the pedestal to point to the right degree on the divided circle, will allow of this adjustment being made with facility and accuracy at once*. Lastly, move the frame, H, backwards or forwards till the moon be brought to her true distance from the earth at the time of adjustment, by means of the scale on the slips; and set the enlightened half of the moon towards the sun, that is, make the plane of the line separating the light dark halves perpendicular to the sun's direction.

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* This dial is, of course, not essential, and may be dispensed with if the earth be *large enough to admit of meridians being drawn on it, but we would* advise its adoption, even in the latter case: the nicety required to construct it properly is a good exercise, and it materially adds to the merits of the Lunarium.

The board may be made to slide between fillets of wood nailed down on one much larger. These fillets being struck out, arcs of concentric circles of such radii, that the path of the earth, when moving between them, may be the circle described by the earth at its true proportionate mean distance, deduced from that originally assumed for the earth and moon: these fillets may be spaced out into *days:* this addition to the Lunarium will enable it to show that the curve described in space by the moon, is not a nodated cycloid, as common orreries show it, but one slightly undulatory, and always concave to the sun.

The young learner must be impressed with the characteristic of this instrument, in which, indeed, its great merit consists; it is only *true* for one fixed position, and will not admit of the moon's being made to revolve round her *whole* orbit, for before she has moved through one twentieth part, her mean distance and the place of her node has altered, but by the simple and easy adjustments which a common ephemeris will enable him to make in as short time as is requisite to read these instructions, he has the earth and moon in their real positions, and by moving the moon for a short arc in her orbit, he sees the *direction* of her motion and its results. But if he look through the hole on K, he must bear in mind that he is no longer an observer from the earth, he must transport himself in imagination to the sun, or at least to the point in the earth's radius vector, represented by the aperture on K. He will see an eclipse of the *earth* when to an inhabitant of it the *sun* is eclipsed, and he will observe an eclipse of the moon by the *body* of the earth, instead of its being obscured only by passing through the shadow of our planet. By turning the dial and earth round on the axis, and keeping his eye in the plane of the former, he may observe the moon's rising and setting, and the hour at which it happens, and, in short, with care and skill he may acquire correct notions of the cause of all the lunar phenomena.

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* The degrees on that circle must be numbered accordingly, but the means of ascertaining where they are to commence will be best understood by looking at the instrument; it should be contrived that the small index should point when the adjustment is made to the degree of the circle on A, seen in the axis *of H: when, therefore, the moon is in her nodes, the index on the pedestal must point to 0°* or 180°, no description can make these niceties intelligible to the dull, and they are not needed by the acute: all that can be said is, that the moon's axis *must* be perpendicular to the *ecliptic*, and not to the plane of H, otherwise there will be a discrepancy between the moon's *real position* and that which it ought to have, as given by the horizontal circle on B.

In conclusion we would observe, in answer to those who might consider the subject and our mode of treating it too elementary for our Magazine, that although it may not interest them personally, yet as no child could make or adjust such an instrument without assistance, and as every one is directly or indirectly interested in the instruction of young people, the paper and the toy it describes are not so inconsiderable as they might at first appear.

THE PROGRESS AND STATE OF SCIENCE IN BELGIUM.

In our opening number we announced our intention of specially devoting a portion of our Journal to the communication of whatever might appear most generally interesting in Foreign Science, and especially from time to time to give such abstracts as we might be able to obtain of its general condition in the several countries of Europe.

This pledge we have partially redeemed by the various notices we have inserted, referring to foreign discoveries and researches. We hope to fulfil the same promise to a greater extent in the present article. We have been kindly favoured with a copy of the Bulletin of the Royal Academy of Sciences at Brussels, containing the detailed proceedings of the public Séance of Dec. 16th, 1835; and shall present our readers with what has appeared to us the truly interesting Report, delivered on that occasion by the perpetual secretary, M. Quetelet.

In doing this, we have thought it best to adhere closely to the author's own words, and, instead of attempting any compilation of our own, to present his luminous and often eloquent views in a translation, if not always strictly literal, yet always following the tenour of his reflections. We think it right to apprize our readers of this, in order to account for an occasional style of expression which would not perhaps be adopted by an original English writer. This, however, we do not doubt, will rather be regarded by our readers as characteristic, and as only tending to bring more vividly to their minds the actual tone of scientific views at present prevailing in Belgium. Without further preface then we commence.

Report of the Permanent Secretary on the Labours of the ANCIENT IMPERIAL AND ROYAL ACADEMY OF BRUSSELS.

In some introductory remarks, the distinguished author gives a rapid sketch of the progress of arts and sciences in Belgium in former centuries:—their flourishing condition under Charles V., and subsequent decline, and almost total extinction. The dawn of a better state of things, however, approached:—

"*It was under* the auspices of Maria Theresa, of glorious memory, *that the Imperial* and Royal Academy of Brussels was formed. The few really well-instructed men that Belgium contained, united themselves with several foreigners of distinction, and the language of science was once more heard amongst us.

"The labours of this learned body were crowned with a brilliant success, and obtained great favour with the nation, who well understood that the opinion that would be formed of her by foreigners, would henceforth depend on the esteem in which an assemblage of men would be held, who might be considered as her representatives of national intelligence. Unhappily, this success was of short duration.

"A revolution, which broke down the greater part of the props of the ancient social edifice, and renewed the political constitution of many states, also changed our destinies.

"The Academy of Brussels was suppressed; its members were dispersed; and when, at a later period, we became a part of the great body of the French empire, of which the whole intellectual life appeared concentrated at Paris, it might be asked with reason, whether Belgium had not again fallen into its ancient state of torpor.

"The Imperial and Royal Academy of Brussels, whose important services are to this day too little appreciated amongst us, appeared then but for an instant, and as a prelude to a new era which was soon to open before us, and to restore to us, with our ancient liberties, our former taste for the arts, literature, and science. When in 1816 this learned body was re-organized, the few members who were yet living, were called upon to compose it; but some had expatriated themselves, and others were for the most part too aged to be able a second time to co-operate in the intellectual regeneration of their country. At the present time these men of another age have successively become extinct.

"The new academy, in creating an annual public session, was desirous of consecrating the memory of the 16th of December, the day on which the ancient Academy of Brussels was founded, and which, by a happy coincidence, is also the birth-day of our august monarch; and they have deemed that they could not with more propriety usher in their public session, than by giving a rapid sketch of the labours of its predecessors, as constituting the finest eulogium that could be offered to their memory.

"It is to me that has been intrusted the honour of paying this tribute of gratitude,—this sacred debt, which is indeed also that of the nation.

"I will not here stop to retrace the origin or the first labours of the Academy, the recital of these may be found detailed in the volumes which commence the former, and the new series of our memoirs.

"I shall only speak of its foundation, with a view to make that character of grandeur and magnificence which was imprinted on it, and which would do honour to the most enlightened governments, duly appreciated. Its august foundress had well perceived, that in order to restore the sciences in a country where they had nearly fallen into complete oblivion, it was necessary to encircle with honours, and worthily to recompense those who cultivated them with success. She constituted the Prince of Stahremberg her minister plenipotentiary to represent her in the Academy, in the capacity of protector; and the chancellor of Brabant was invested with the presidency. The royal library was assigned as the ordinary place of assemblage. The Academy had moreover the enjoyment of that rich collection, which had formerly belonged to the duke of Burgundy: permission was granted it to use as its great seal the arms of that illustious house, and thus of associating its name with those of the brightest recollections of our national history. Funds were liberally accorded for the printing of the memoirs, for remunerating contributors, and for scientific travels. Pensions were created in favour of superannuated members, or for such as had distinguished themselves by their activity.

"'Lastly, to give a further mark of the particular esteem that we accord to useful talents, and to those who cultivate them with success,' said the empress in her letters patent, 'we decre that the quality of academician shall communicate to all such as shall be decorated with it, and who shall not be already ennobled by their birth, the distinctions and prerogatives appertaining to the estate of personal nobility, and this in virtue of the act of their admission into this society.'

"If we quote these words it is not to attach importance to ancient prerogatives, but to give a clear understanding of the powerful assistance which science received at a time when these prerogatives were everything in the eyes of the world.

"The academy, however, received a still higher privilege, an inestimable benefit for the learned,—I mean the liberty of the press, that mother of thought, which then appeared as a consoling phenomenon, on emerging from our long night.

"So many combined advantages would naturally create an ambition of being an academician; thus a noble emulation spread throughout Belgium, and it was not long before talents were seen to arise, which would have remained stupified without stimulants of such energy. Five volumes of memoirs were published by the Imperial and Royal Academy of Brussels, during its short existence; as were also many volumes of prize-memoirs. A detailed analysis of these scientific and literary works would perhaps become tiresome, but it may be interesting to examine into the useful consequences they were of to Belgium.

"If we consider, for example, the physical sciences, we shall observe, that in order to judge of their advancement in a country, one may take for a standard, the height to which the study of mathematics has been carried. Mathematics is the language in which natural phenomena are expressed, and valued numerically, when they have been duly studied and reduced to their most simple elements; and in general, the difficulty which most branches of science experience in having their phenomena translated into this language, only tends to show the feeble degree of advance they have made.

"In adopting a like scale we find since the date of the earliest publications of the old academy, an immense progress in Belgium. In fact, the birth of the infinitesimal calculus had, as we have already said, followed close upon the decay of science in our provinces; it had attained the most rapid growth, and that all-searching instrument, the powers of which were tried by removing, as if by enchantment, the thick veil which covered the finest secrets of the system of the world, had not even attracted the attention of Belgium. After having reduced, if one may so speak, the heavens within its dominion, the infinitesimal calculus had made the happiest excursion through the fields of physics, and attacked directly the most beautiful problems of that science, in which we were yet studying works that were quite out of date.

"The commandant of Nieuport, in the second volume of the *Ancient Memoirs of the Academy*, was the first to show that the higher branches of analysis had found an interpreter in Belgium: he at once accomplished the solution of many important problems with which geometers were then occupied, and his labours put him in communication with d'Alembert, Bossut, and Condorcet. Such relations not only do honour to the learned person who is the object of them, but also to the country to which he belongs.

"So fine an example hardly found any imitators. Mr. Bournouns was the only one in the Academy, and one may say in Belgium, who occupied himself with researches of the higher analysis, but with much less success than the commandant of Nieuport. The ancient university of Louvain, in its course of instruction, scarcely went beyond the rule of Cardan for the resolution of equations of the third degree; and as for astronomy, it still held to the vortices of Descartes, although many of its professors began to occupy themselves with the laws of attraction. As for astronomical observation, it absolutely did not exist; it was to foreign men of science, who associated themselves with the first labours of the academy of Brussels, that the only observations really worthy of having been made in this country, are due: these have been registered in our ancient memoirs, where we meet with the names of Messieurs Pigott, the Count of Bruhl, the Baron Zach and Lalande. When this last astronomer made the circuit of Europe to visit the observatories, he did not dissemble his astonishment at not finding amongst us any traces of his favourite science. 'In the Austrian Netherlands, now French,' he writes, 'astronomy does not appear to have been cultivated:' he then adds, 'the only observer of this country is an English gentleman of the name of Pigott.' This learned person is indeed established among us, and he made at Louvain, Brussels, Ostend, Tournay, Luxembourg, and Hoogstraeten, various observations on the satellites of Jupiter; he also took the meridional altitudes of a great number of stars, by means of one of Bird's Quadrants, which had been intrusted to him by the Royal Society of London. These observations were undertaken with a view of co-operating in the construction of a good map of the country, which was desired by the government,—a map which at the present day is so much wanted, and which forms a blank, but little honourable it must be confessed, amidst those geodæsical labours which have been accomplished by our neighbours.

"It was, again, the ancient academicians of Brussels, who contributed to spread in Belgium with the greater ardour the novel and brilliant discoveries of the physical sciences; nor were they rendered less useful by the applicability of their knowledge to the study of our own country, with which they were occupied with the greatest zeal; and it is that which makes the collections of our memoirs so precious to the learned, who may wish to study our provinces in a scientific and literary point of view. Amongst the members who most distinguished themselves in the physical sciences, must be mentioned the Abbés Mann, de Needham, de Witry, and Dr. Godart. The first of these in particular was remarkable for the diversity of his works; it is true they must not be considered, even with reference to science, as being very profound, but ingenious views, and sometimes the most happy conceptions are there found. Thus this learned man has well hit upon the relations which exist between the appearances of the aurora borealis, the movements of the magnetic needle, and the quantities of atmospheric electricity, affinities which have so much engaged, in these latter times, the attention of the most distinguished natural philosophers. He had likewise formed very correct notions on the method that should be pursued in the study of meteorology, a science in which but little advance has been made in our time, notwithstanding all the labours that had been undertaken in order to accelerate its progress.

"The Palatinate Meteorological Society had just been organized at Manheim, and addressed itself to the principal learned bodies in Europe, to ask their co-operation in the vast system of combined observations which it proposed to execute; it also addressed itself to the Academy of Brussels, and the Abbé Mann was selected to answer the appeal of the learned Germans. He acquitted himself honourably of his mission; and even now his observations are consulted with profit, and cited in most treatises on natural philosophy. Many other members of the academy occupied themselves equally with meteorology; and the learned professor Van Swinden, enriched our ancient collections by a memoir containing his observations made in 1778. It is to these collections that we must refer, to become acquainted with our earliest documents upon the temperature, the variations of atmospheric pressure, and everything relating to our climate. It is also there that we find the only three observations on the declination of the magnetic needle, which have ever been made in our country, even to the present time. Chemistry was not neglected, but it encountered many difficulties before it was enabled to assume, among the sciences, the important rank which it at present occupies. M. de. Bennie undertook to analyze the different soils around Antwerp, with a view of finding some method of improving our heaths. Many other members also wrote on questions of chemistry of general utility, and especially those relating to our agricultural industry and to our mineral waters.

"A spirit of observation is among the qualities which distinguish the Belgian people, thus natural science has always possessed for them a powerful attraction: it is sufficient to cite the names of some of our predecessors to show that it was not neglected in the ancient Academy. It is necessary to observe here that the greater part of the memoirs which were published on natural science, concerned Belgium; for the good of the country has always been, with the Academy, the central point towards which all their researches tended. It is also to be remarked, that the members have rarely attempted general theories and the more abstruse questions of science; they have limited themselves to more modest labours,—they endeavoured to collect useful materials, leaving to more enterprising architects the care of construction.

"It is to this epoch that we must refer the earliest researches on the geological constitution of our provinces, and on their fossils: these inquiries have latterly taken the happiest turn, and the present Academy, in the judgment of the most able geologists, may present them among its most honourable titles. One of our learned members, who has taken an active part in these labours, is about to make their importance more justly appreciated than I should be able to do.

"Physical geography and rural economy were also properly attended to. Among the questions which were treated, the most advantageous means were equally sought for clearing the heaths of our Ardennes: the former state of maritime Flanders was examined, the successive changes which had been produced there, and whatever related to the tides along our coasts.

"Again; the Abbé de Nelis and the Marquis du Chasteler, who united varied knowledge with an elevated mind, treated many subjects which have since found an important place in political economy, a science, whose name bearing the stamp of novelty, has not even yet obtained an easy access into all minds. The question to know whether, in a fertile and well-peopled country, large farms are useful or hurtful to the state in general, was treated by the Marquis du Chasteler and the Abbé Mann, in connexion with a discussion which had arisen between this latter savant and the English economists. The reasons of our academicians were set forth in a very striking manner, which even now possess great interest, particularly to Belgians, because the subject is treated in a point of view especially applicable to them. The Abbé Mann, who, nearly alone among our academicians, had an inclination to touch upon questions of a general tendency, did not draw back from one of the most difficult, which forms as it were the basis of social science, and which has demanded the concurrence of the cleverest modern political writers, before it could be regarded in its true point of view; I speak of the question of population. It is true that he did not really attack the difficulty; for, with the pastor Meuret, regarding the increase of the population as an incontestable good, he only occupied himself with indicating the means of attaining it.

"If I have spoken of this labour, it is to show that the importance of the political and moral sciences had been understood by the ancient Academy of Brussels, and to explain at the same time what opinion still prevailed here on a leading question, in so populous a country as ours."

Report on the Progress and actual State of Geology, and the Sciences connected with it, in Belgium.

"In requesting for the public session of this day, a report on the progress and actual state in Belgium of geology, and the sciences connected with it, the Academy desires to acknowledge and to show to the country, that part of the debt which has already been paid to these sciences, and also that which is yet due to them.

"In giving myself up to the inquiries which this work demanded, I have obtained such satisfactory results as have rendered the task which had been imposed upon me as pleasant as it is honourable. Many others might have fulfilled it with more talent, but none, I venture to say, with more sincerity and gratification.

"The study of the mineral kingdom has been long neglected in Belgium, at which there is much reason to be surprised, when it is remarked, that it is, in proportion to its extent, one of the richest countries on the globe in mineral substances; that the most precious of all, pit-coal and iron-ore are not only spread there with astonishing profusion, but known and worked from so distant a period, that the most diligent investigators of our archives cannot yet determine the limit beyond which the discovery must be dated.

"Notwithstanding, the working and treatment of mineral riches, though less difficult formerly than at present, already demanded considerable knowledge, which really did exist in those of our provinces where these kinds of labour were pursued; and both in Belgium and Germany the miners practically cultivated mineralogy and geology, much before Werner had studied, with them, the composition of the terrestrial crust, which they dug with so much courage and talent. But we worked and treated our minerals as we cultivate useful vegetables, in our fields and in our green-houses, that is to say, with an art and a success which have never been contested, studying nature unceasingly, in those of her productions from which we could extract use; seeking and discovering the means by which we might procure them in the greatest abundance; in a word, always observing, but reading little, writing still less, and leaving to others the empty pleasure of imagining systems.

"This manner of studying natural science has been sufficient for the industrious wants of the epoch over which we are now casting a rapid glance; but from the end of the last century it could no longer satisfy the man of taste, curious to know all the beauties of nature, or the philosopher, eager to seize some one of the laws which preside over this admirable assemblage of things. The moment was come for the Belgians, as well as for all educated people, to collect, describe, and class all the productions of their soil. They began then to arrange some collections; and some among them wishing that their successors should enjoy the fruit of their labours, have made known the results in writings, which all form a part of the *Memoirs of the Ancient Academy of Brussels*, and which are now read with interest,—an honour for the former Belgians, who, in rushing on the career of geology, have drawn their fellow-countrymen along with them.

"M. Robert de Limberg was the first who leaped the barrier. He presented, in 1770, to the Academy, the numerous observations which he had collected around Theux, his native town; he afterwards extended his researches upon more distant points, and gave an account of them in a memoir that he read four years after.

"In 1778, M. de Launay read his memoir upon the origin of the animal and vegetable fossils of the Belgian provinces, preceded by a discourse upon the theory of the earth.

"In three memoirs presented successively in 1777, 1779, and 1785, the Abbé de Vitry opened his mineralogical and paleonthological researches in Tournay and the Austrian Hainault.

"M. de Burtin produced, in 1784, his *Oryctography of the Environs of Brussels.* In this work, very remarkable for the epoch at which it was written, the author makes known the minerals which he has found in a circle of five miles round Brussels, describes and represents upon the thirty-two plates, which accompany the text, a part of those remains of marine animals accumulated in such abundance in the sands, and in the hardest rocks of the land which he has so well studied, establishes that the greater part of these animals cannot be related to those species which are at present alive; that only some of them are analogous to those found in the torrid zone; that they have been lodged at the bottom of a sea which covered these fields, where now rich harvests abound; and that they have been buried tranquilly in the spot where they lived. He deduced from these facts, at present admitted by all naturalists, very judicious consequences upon the theory of the earth.

"The mineralogical observations from Brussels, by Wavre, to Court Saint Etienne, that the same author has presented, the same year, to the Academy, bear equally the stamp of true talent.

"If, during the twenty-five years which have followed the publication of M. Burtin's work, there has not appeared any geological work, which can be compared to his, the cause must undoubtedly be sought for in the political situation in which the country has been.—Who does not indeed know that calm and stability are as necessary for scientific studies, as for the speculations of commerce and industry? We do not therefore find, in the archives of geology relative to Belgium during this period of a quarter of a century, anything except a few small pieces of that veteran of Belgian geologists, M. Dethier, who has pointed out to the attention of naturalists the presence of extinct volcanoes in the Eifel, a country, now so celebrated in the records of science, and of which a part then belonged to our provinces; some interesting remarks, by M. Baillet, upon the alum-mines of the province of Liege; on the total slip of a mountain of freestone, in the same province; upon the lead mines of Vedrin, of Dourbes, of Vierve, of Treigne (a province of Namur) and upon that of Sirault, (a province of Hainault) upon the mine of calamine of the Vieille-Montagne and upon the arsenical pyrites of Enghien; two memoirs on the mines of pit-coal of the province of Hainault have yet been published during the period of which we are now speaking, the one by the prefect of the department, the other by M. Gendebien; but considerations upon industry and commerce occupy more space than geological researches. It is also just to notice the publication, at Brussels, during this same period, of the systematic distribution of the productions of the mineral kingdom, presented by M. Launay, to the Academic Session of the 4th of June, 1788, of the *Mineralogy of the Ancients,* published afterwards by the same author, and of the *Essay upon the Study of Mineralogy,* by Rozin.

"Towards the end of 1808, M. D'Omalins, who had already published some remarks upon the minerals and the rocks of Belgium, brought out under the modest title of *An Essay*, the geological description of the north of France. He there divides our land into seven formations, which he respectively designates, in going from the lower to the upper, by the names of *trappeenère, ardoisière, bituménifère, du grès rouge, du calcaire horizontale, du grès blanc, du terrain meuble;* he sketches rapidly the principal geographical and geognostical characters of these groups, and only leaves to those who shall seize the pencil after him, the care of shading this vast drawing. After having described the country of his birth, he undertook a geological map of that immense empire to which his country had been united, and presented, in 1813, to the Institute of France, the first results of this enormous labour, with a cut representing the structure of the lands, which extend from our Ardenne, to the mountains in the centre of France. The first who hastened to follow the steps of M. D'Omalins was M. Bouesnel. From 1811 to 1815 he published, successively, seven memoirs; upon the position of the beds (*gisement*) of the minerals in the department of Sambre and Meuse; upon the mines of iron of the middle Sambre and Meuse; upon the zinc-mine of the Vieille-Montagne; upon the pipe-clay of Ardenne; on the minerals produced from the copper-mine of Stolzembourg; upon the iron-ore in the forest of Soigné; and upon the mines of pit-coal of Flence. These useful labours, known to all the Belgian geologists, cause it to be deeply regretted that since 1815, their learned author has only published one single geological piece, that in which he described, in 1826, the calamine of Santour, near Philipville. We are now arrived at the epoch when natural science has taken, in Belgium, a flight which has been sustained and developed even to the present day. Previous to indicating the results which it has produced, we must go back to the causes which have excited it.

"From the first period of its restoration,the academy had resolved to bring forward for competition for all the provinces of Belgium, the description of their geological constitution, that of the species of minerals and fossils that they contain, with the mark of their localities, and the synonymy of the authors who have already written of them. Plainly put as it is, this question obliged all the competitors to follow the only route which could conduct them to satisfactory and durable theories in natural science; it only demanded from them the observation and classification of facts; for, said Cuvier, 'it is that which gives to natural science its peculiar character, and which, taking from the field which it traverses, every obstacle and every limit, promises certain success to every reasonable observer, who, not yielding himself to rash suppositions, follows the only route open to the human mind, in its actual state.' The creation of special professorships of mineralogy and geology in the three universities of Belgium, at the Athenæum of Namur, and at the School for Medicine, and the Museum of Brussels, has powerfully contributed to propagate the taste for these sciences among our young fellow-countrymen: the establishment of collections of minerals and rocks in these towns, has seconded this intellectual movement, in giving to the scholars, without trouble or expense to them, an idea of the wonders of nature, and in leading them to go to study and admire them in their true places. The government had also taken another step eminently fitted to attain the same end; it had directed the construction of a correct geological chart of Belgium; it had confided the compilation to MM. Van Breda and Van Gorum, who were to act in concert with MM. D'Omalins and Bouesnel, and who had charged with the determination of the limits of the lands two men well worthy of their confidence; MM. Schulz and Van Panhuys. The first died *at an early age, leaving* at least, as a proof of his talents in the *graphic arts, a collection of views* of the Grotto of Remouchamp. Nothing remains to us of the second but the remembrance of his learning, his zeal, and his accuracy, and the regret of having lost with him the nearly-complete geological charts of the provinces of Hainault and Namur. If the events of 1830, had not stopped the great work of which we have just spoken, Belgium would now possess, like England, and many parts of Germany, its Geological Chart, so impatiently expected by science and industry. The government has acknowledged the necessity of recommencing immediately, and of pursuing actively the execution of it; and if it has not yet adopted the measures most proper to attain this end, this delay can only be attributed to the difficulty of finding a sufficient number of able naturalists at this moment in which so many new institutions claim the co-operation of all the talent which Belgium possesses. Let us however hope that these difficulties are about to disappear with the circumstances which have given rise to them, and that we shall soon have the pleasure of hearing that the construction of our Geological map is recommenced with desirable activity.

"The impulse given by the academy, and by the government, to the study of the sciences of observation, and particularly of those whose object it is to make us acquainted with the composition of this little, but most interesting part of the crust of the globe occupied by Belgium, has been seconded by so great a number of naturalists, that I cannot venture to present here a complete enumeration of the works with which they have enriched science, I shall only name the principal, beginning with those which have been composed without the influence and the patronage of the academy, although their authors are almost all of the number of its fellow-labourers.

"In spite of the service which he had rendered by the publication of his Essay on the Geology of the North of France, M. D'Omalins thought that he had not yet done enough in favour of those of his fellow-citizens, who had taken him for their guide in their geological studies; and wishing to facilitate more and more the execution of the labours destined to complete his work, he published, in 1822, a Geological Map, composed in 1813, of France, the Low Countries, and some neighbouring states; in 1828, a collection of his memoirs corrected, and all new discoveries in the science, since the first edition, inserted; in 1831, his *Elements of Geology*, of which the edition has been so rapidly sold that he was obliged to bring out another in 1835; in the interval between the two publications of this work he has also published, in 1833, his *Introduction to Geology*, comprising remarks on astronomy, meteorology, and mineralogy.

"M. Van Breda, who has powerfully contributed to exploring the geology of Belgium, published in 1829, with Mr. Van Hees, an account of the bones of mammiferous animals found in the rock which forms the platform of St. Pierre, near Maestricht; he composed, in 1830, a memoir, not yet published, upon Flanders, and he would no doubt have communicated to us the result of his researches upon the fossils of some of our lands, and upon the bones buried in some of them, if the events of 1830 had not interrupted the course of his laborious and devoted studies.

M. Levy, who during his stay, alas! too short among us, has done much to propagate the taste for mineralogy, by the brilliant as well as profound manner in which he has professed it in the University of Liège, has only left as a remembrance to the country which had adopted him, a memoir upon various mineral substances, of which one is new,—the bed of lapis calaminaris, of Vieille Montagne.

"M. Schmerling has principally investigated that period so interesting *in the natural history of* the globe, characterized by the appearance of man *upon its surface: the great* work that he is publishing at this moment, upon the fossil-bones of the province of Liège, and his immense collection of the remains of those animals which the earth supported at the period of which I have just spoken, are incontestable titles to the national gratitude for this indefatigable naturalist.

"M. Moren, whose studies have principally for their object physiology, zoology, and botany, has also occupied himself in researches which interest the geologist; he published, in 1832, his observations on the human bones found in the peat-bogs of Flanders, and in 1834, on the fossil-bones of elephants, found in Belgium.

"In giving publicity to the numerous observations that he has made upon the lead-mine of Lougively, which he directs, M. Benoit has contributed to extend the circle of our knowledge of the Ardenne, that part of Belgium which is so interesting to the geologist.

"M. Nyst has just made a new step in the study of our tertiary lands, in printing his researches upon the fossil-shells of the environs of Antwerp.

"Many learned foreigners are also joining their efforts to those of the Belgian geologists, and adding some materials to those already collected by them, to serve for a geological description of Belgium. We have much pleasure in noticing the services rendered by MM. Faujas de Saint Fond, Bory de Saint Vincent, Dechen Oeynhausen, Œninghans, Fitton, Lajoukaire, De-ville-neuve, Rozet, and Clère.

"Although M. Vandermaelen (Philippe) has not published any original work on that branch of natural science which is the object of this Report, we ought to acknowledge that he has rendered great services to it, in organizing in his establishment, at present known by all those who cultivate the sciences in Belgium, collections which are the greatest help to the naturalist, in instituting courses of lectures, destined to spread, among all classes of society, a taste for the beautiful and the sublime; in drawing towards him to concur in the accomplishment of his generous and philanthropic views young men, whom he afterwards sends to traverse distant countries, there to complete their instruction, and to collect materials of every species, for the construction of the monument which he raises here to the sciences of observation.

"The extraordinary and daily increasing developement of industry, during the recent period that we are surveying, has had also a great influence upon the progress of the mineralogical and geological sciences; and these sciences have, in their turn, lent their salutary aid to the branches of that industry which is exercised on mineral substances. To develop this truth, which is not now doubted except by ignorance, would be to insult the understanding of those who listen to me; I shall therefore limit myself to add, that the members of the mining body, principally destined to enlighten with the torch of theory, the arts of working and treating minerals, have seconded by the communication of elements drawn from practice, the efforts of the geologists who have written on the Belgian Provinces in which mines exist. It only remains for me to speak of the geological works which have been composed under the influence or patronage of the academy; but here a new reserve is imposed upon me, for if I have ventured to give an opinion, or rather to recall the judgment already formed by the public, upon those rather ancient, and generally-extended works which I have cited above, I cannot act in the same manner in regard to those with which I am now to be occupied. It is to public opinion that we must exclusively defer, for the confirmation or contradiction of our judgment, for appreciating our works, and for assigning them their suitable place in the archives of human knowledge,—the appeal of the Academy has been heard and understood; of nine memoirs which have been remitted to it, in answer to the question of geology, successively applied to the provinces of Hainault, Namur, Luxembourg, Liège, and Brabant, it has adjudged seven prizes to their authors, (five first and two second) whom I feel obliged to name in following the chronological order of their works. They are MM. Drapiez, Cauchy, Steininger, Englespach, Larivière, Dumont, Davreux, and Galéotti. Although I do not permit myself to speak of these writings, I cannot, however, do otherwise than mention here, that the new and ingenious views presented of our ancient lands by M. Dumont in his "*Geological Description of the Province of Liège*, have just received a striking confirmation, by the verification which the Geological Society of France has made of them this summer; and by the memoirs which the learned Murchison has published upon the northern part of Great Britain.

"Besides these very long works, we must also remember that there is in the nine volumes of the *New Memoirs of the Academy*, the 'Dissertation on the Stratiform Trap-Rocks,' of M. Kickx; the relation of a 'Voyage to the Grotto of Han,' made in 1822 by MM. Kickx and Quetelet; a notice on '*Les Pierres à chaux Hydraulique*, of the provinces of Hainault and Namur,' by the Editor of the present Report; 'Observations on the Divisions of the Lands,' by M. D'Omalins: and in the *Bulletin des Séances*, published by the Academy since 1832, a great number of isolated observations which we offer to those who will one day come to take a part in the interest for science and art.

"We may then consider as nearly terminated tho mineralogical and geological description of the five above named provinces; that is to say, of that part of Belgium which presents the greatest interest under the relation in which we here regard it.

"That of the province of Antwerp has been the object of one of the prize questions of 1836; and that of the two united Flanders, is already proposed for the prize of 1837. When it shall have obtained satisfactory answers to these two questions, the academy will doubtless judge it necessary to give its attention to the revision which will probably be needful, at least for the first memoirs that it has received; and to call all the Belgian geologists to a great meeting, whose object will be to dispose and re-unite all these works digested by different authors, and at periods whose extremes are separated by a very long interval. Then Belgium will no longer have cause to envy neighbouring nations, which have made most progress in the study of those lands whose surface she occupies. In the mean while, and wishing at the same time to show the extent of the works executed at this day upon the geological constitution of this country, and to satisfy, as much as is in my power, the natural impatience of persons, who, by taste or necessity, have an interest in knowing its mineral riches, I have undertaken summarily to unite all the mineralogical and geological observations which are found scattered in the numerous writings which have been recently named, and I have the honour to submit to the judgment of the academy, 'A Synoptical Table of the Minerals and Rocks of Belgium, considered under a Mineralogical, Geological, Geographical, and Technological relation.'

"But it is time to terminate this nomenclature of scientific works, whose barrenness will be, I hope, in part excused by the motives of expediency which have influenced my pen. I hasten also to call the attention of the public for an instant to the 'Geological Description of the Province of Brabant,' whose author we are about to crown. His memoir will be immediately printed; it will be read with the most lively interest, we cannot for a moment doubt, not only by the friends of science, but by all the educated men of this capital. These wish to know what the country formerly was which they inhabit, they *will see that in the place of* populous cities, of delicious gardens, and smiling *fields which surround* them, a vast and deep sea covered rocks which now constitute the soil of Ardenne, and beat with its unconquered waves those which disclose to us the summits in the environs of Hal, of Genappe, and of Jodoigne; they will learn by what a series of revolutions the deep valleys, which furrowed this ancient soil have been overwhelmed, and its high mountains covered by successive deposits, which have at last produced the level soil which we at present tread.

"M. Galeotte will not himself be present to receive the only recompense which we are able to grant to his useful and fatiguing labours. Just returned from a scientific excursion in the countries of Germany, where he might make trial of his useful talent, he is gone to Mexico, to exercise it there in a newer and vaster field, that he may gather there a new harvest of knowledge, with which he will return to do homage to Belgium, and to acquire scientific riches, which he will place in the geographical establishment of Brussels, where he has pursued his studies.

"It is then upon some steep peak of the Andes, upon the burning crater of some volcano, or in the bottom of some mine in the New World, that he will receive the palm which we are about to decree to him. If it shall too forcibly recall the memory of his absent country, if some regret shall mix its bitterness with the joy which shall fill his heart, may he think that we here apply to him those beautiful verses of the poet Millevoye,

"Gloire à l'homme inspiré que la soif de connaître
Exile noblement du toil que l'a vu naître,
Les tranquilles honneurs, les trésors, l'amitié,
A ses projets hardis tout est sacrifié.

Les travaux, les dangers, son zèle les surmonte;
L'obstacle, il le combat, le trépas, il l'affronte,
Faut-il franchir les monts? faut-il dompter les flots?
Son intrepidité ne craint que le repos.

The sitting was terminated by the distribution of medals decreed to the competitors of 1834 and 1835. The permanent secretary successively proclaimed the names of the laureates, who received the medals from the hands of the directors.

BOTANICAL RAMBLES IN THE VICINITY OF DOVOR.

No. II.

In this ramble I shall propose taking the reader again over a part of the ground we beat in our last, that is to say, under the Castle-cliffs to the zig-zag path by the Station-house. Here we shall break upon new ground, and proceed under the South-foreland cliffs to St. Margaret's, and thence returning over the downs, by the lighthouse and the castle-hill, we will digress a little from the straight road when we reach the latter spot, to examine the fields and lanes about Charlton. As we proceed, a great many of the same plants mentioned in my last will come before us again, but I shall not refer to them excepting in a few instances, where I may have something to observe. My object is to point out the localities of plants I have not before mentioned; it must therefore be expected, that the more we ramble, the fewer we shall have to note.

Proceeding then under the Castle-cliffs to the winding path by the *Station-house, we will here begin* again to make our observations. The first botanizing ground we shall come to is some slopes situated on a ledge of the cliff many feet above the shingle, but still not difficult to be reached if a little more than usual exertion be resorted to in the climbing way. When once upon the ledge there is plenty of space, and a luxuriant bed of vegetation. Here we shall find common Juniper (*Juniperus communis*), and observe the common Ivy (*Hedera Helix*), trailing over the slopes and matting itself with the brambles and other shrubs which fall in its way. It does not climb up the surface of the cliff and show itself in that luxuriant manner as on the ruins of Kenilworth Castle, and at other places, where the beauty of the scene is much enhanced by its appearance. The situation here may be too exposed, or more probably, the chalk-cliff is not sufficiently firm for the roots of the ivy to find a holding-ground. The Horse-shoe Vetch (*Hippocrepis comosa*), which we have before observed, becomes more plentiful here, and is found in abundance on some of the slopes. The South-foreland cliffs, which we shall soon reach, are very perpendicular, and their height gives an imposing grandeur to this part of the coast. They are free from any kind of vegetation, being one unspotted mass of chalk, which ever looks beautiful, and its pure-white surface is seen from afar, and helped to designate our Island—Albion. Hawks, ravens, and various sea-birds seem to have monopolized the spot, but nestle so high up that they would not be observed, did not discordant screams give notice of their presence, and call the attention of the passer-by. At the foot of these cliffs we shall find a few small bushes of Sea Buckthorn (*Hippophae rhamnoides*), on some sloping banks near the tide-mark, and a little further on, the slender-flowered Thistle (*Carduus tenuiflorus*), shows itself growing at the foot of the cliff*. We shall now advance to St. Margaret's, where, as I have before stated, the cliff sinks to a mere bank, and rises again very soon. Here we shall observe a considerable bed of shingle running in, around which are a few cottages, a station-house, and a clean public-house. A solitary bathing-machine stands on the beach. The bulk of the village of St. Margaret's lies inland—this may be termed the port. On the eastern side of this sequestered spot, on the slopes of the cliffs, large beds of common Fennel (*Fœniculum vulgare*) will be observed, which when in flower, have a beautiful golden appearance. Chalk-cliffs in the vicinity of the sea are the usual locality for this plant, but I have my doubts whether it be truly wild in this spot, it having very probably escaped from some cottage-gardens close at hand, and finding a soil it liked well, has spread itself over the extent of ground it now covers. Some of the stems I noticed to be from five to six feet high, with the umbels unusually large.

Returning over the top of the cliff we will first make for the South-foreland lighthouses, in the fields around which we shall find the common Basil Thyme (*Acinos vulgaris*), growing in extraordinary abundance, the surface of the soil in some places receiving a tint of blue when the plants are in flower. The rare, round rough-headed Poppy (*Papaver hybridum*), grows in the neighbourhood of the lighthouses, particularly in a field between them and the cliff. Our road will now lead us to the station-house above the zig-zag path, frequently referred to before, and as we pass along we shall observe short-awned annual Darnel (*Lolium arvense*), and narrow-leaved pale Flax (*Linum angustifolium*). The way from this station-house to the castle-hill having been pointed out in a former ramble, we will proceed at once to the latter spot, and here I hope the reader will excuse me, if instead of going home direct, we make a little excursion over the fields to the right, to the rivulet near Charlton, and thence into the town through the lanes. We shall first pass over a series of cultivated fields, where an opportunity will offer of observing the smooth narrow-fruited Corn-Salad (*Fedia dentata*), Common Nipple-wort (*Lapsana communis*), and dwarf Spurge (*Euphorbia exigua*). Some of these fields have grassy-borders, where the small Woodruff (*Asperula cynanchica*) grows, a favourite plant, common in chalky districts, but rare elsewhere. About Charlton are some shady lanes with high banks, where the following plants grow—Greater Plantain (*Plantago major*), great Bindweed (*Convolvulus sepium*), sweet Violet (*Viola odorata*), Silver-weed (*Potentilla anserina*), common Avens (*Geum urbanum*), common Mallow (*Malva sylvestris*), Herb Robert (*Geranium robertianum*), and Dove's-foot Crane's-bill (*Geranium molle*). The common Traveller's-joy (*Clematis vitalba*) abounds here, as in most calcareous soils. This plant is provincially termed Honesty or Old-man's beard, and in some of our inland counties is a favourite with the poor boys, who cut the stem into pieces about six inches long, and setting fire to one end, smoke them when dry, in the manner of a pipe. The wood bëing very porous, allows the air to pass through with ease, and the mimic smoking of a piece of honesty often leads, I am inclined to think, to the higher accomplishment, that of the tobacco-pipe: many of the old mothers who act as village doctors, affirm that the smoke from the Traveller's-joy, when properly inhaled, is an excellent remedy in cases of Asthma. Near the rivulet, and in damp places along the lanes as we pass home, we shall meet with spotted Persicaria (*Polygonum persicaria*), pale-flowered Persicaria, (*Polygonum lapithifolium*), common Meadow-sweet (*Spiræa ulmaria*), Water Betony (*Scrophularia aquatica*), and square-stalked St. John's-wort (*Hypericum quadrangulum*). On the right of the lane leading from Charlton to Dover, a little before we reach the town, is an old wall, on which the crooked yellow Stonecrop (*Sedum reflexum*) grows, one of those plants so peculiarly constituted, that it will bear the greatest droughts and heats, although exposed to the scorching rays of the sun with but little or no earth for its roots. In Oxfordshire, the cottagers gather this plant, and suspend pieces of it in their rooms by a string, calling it the air-plant, from its shooting and growing for a short period even when thus severed from its roots. We shall now soon reach the town, and I shall conclude this second ramble, as I am sure the reader will not wish to proceed any further at present. W. W. S.

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* For the use of those who add the study of Entomology to that of Botany, I may here remark, that under these cliffs I have taken four species of the blue butterfly, known scientifically as belonging to the genus Polyommatus, as well as the rare *clouded-yellow butterfly*, Colias Hyale. *Four species of the genus* Vanessa abound, the Io, Atalanta, Urticæ, and Cardui, besides several other butterflies. Licinus depressus, and Sylphoides, two very local beetles, will be found under flints and pieces of chalk in August and September, and the rare hemipterous insect, Coreus scapha, is far from uncommon.

MISCELLANEOUS INTELLIGENCE.

New Mail-Coaches; their Cost, Performance, &c.

ENGLAND, WALES, AND SCOTLAND.

THE total number of miles run by the Mail-Coaches annually, is	4,831,870
This is nearly equal to a journey 200 times round the world.	
The number of miles run by them daily, is	13,238
More than equal to one-half the circumference of the world.	
The number of miles run by the London Mail-Coaches only, in one despatch and return, is	7,010
The number of Mail-Coaches required for the service of the Post-office is about	290
Agreeably to the contracts made in the Summer of 1835, and which came into operation on the 5th of January last for a term of seven years, the Mail-Coach (very much improved in construction, safety, and convenience) is furnished, and kept in repair, greased, oiled, and cleaned completely fit for service, in the southern and midland districts (10,124 miles), at per mile run	1$\frac{1}{10}$*d.*
In the northern district (3,114 miles), at ditto	1*d.*
The annual cost of furnishing and maintaining the Mail-Coaches, is	*£21,095 11 11¾
The average amount paid to the old contractors in each of the ten years, ending 31st December, 1834, was	£32,947 6 11
So that there is a difference between the old and the new contracts, and a saving to the public annually of about	£12,000 0 0

[*Return, Post-Office Comm. to House of Comm.* 13*th Aug.* 1835.]

British Association,—Sixth Meeting.

THE following Circular has been addressed to the members of the British Association for the Advancement of Science, (*post paid.*)

"Bristol, July 1st, 1836.

"SIR,—We beg to inform you, that the next Meeting of the British Association for the Advancement of Science, will be held in Bristol, during the week commencing on Monday, August 22nd.

"The Most Noble the Marquis of Lansdowne, the President Elect, will take the Chair on the evening of that day.

"With a view to the formation of the Sections, and other preliminary arrangements, the General Committee will meet on Saturday, August 20th, at Twelve o'Clock.

"It is requested, that Members who may have any Papers, or other communications, to lay before the Association, will state before the end of July their general nature and probable extent, in letters addressed as follows:—*To the Provisional Secretary of the* [Mathematical, Chemical, or other] *Section, Philosophical Institution, Park-street, Bristol.*

"Unless this precaution be attended to, great inconvenience must arise, and valuable papers may be unavoidably rejected in the press of business. Investigations of any considerable length may be most advantageously presented *in abstract.*

"It is also very desirable, that gentlemen who propose to attend should signify their intention early to the nearest Local Treasurer, or to one of the Secretaries for Bristol.

"Information for Members, on their arrival, will be given at the Council Chamber, Council House, at which place there will be an attendance of proper persons for that purpose during the week of the Meeting and the preceding week.

C. DAUBENY, M.D., } *Secretaries for*
V. F. HOVENDEN, } *Bristol.*

It appears, by a postscript to the above, that the fourth volume of the Reports, containing the Transactions at Dublin, in 1835, the Recommendations of the Association and its Committees, &c., "will be ready soon after the middle of July." It is not yet ready (26th July); perhaps it would be impossible, in the peculiar circumstances under which the MSS. for these volumes are obtained, the proofs corrected, &c., to publish them sooner; but as the "Recommendations," &c., contain Propositions for Experiments, Treatises, &c., desirable, at least, to be prepared and delivered in time for the succeeding Anniversary, we submit to the Management of the Association the propriety of not reserving these until the very eve of the meeting, but of publishing them the earliest possible opportunity after the dispersion of the Association,—while the subjects are fresh in the recollection of the members who attended, and giving the greatest possible time for preparation to those who were absent, and to all others interested in the subjects. A sheet, without a cover, would contain all, and might be the last of the series of those very useful *Bulletins* which have been published during some of the meetings[*].

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* By a return, dated 19th February, 1836, the actual amount payable to the new Mail-Coach Contractors for the quarter *ending 5th April, 1836,* is stated to be *£5,239. 17s. 8d.*

German Association, 1836.

The annual meeting of natural philosophers of Germany, will take place at Jena, on the 10th of September.

Prostitution of the Medallic Art.

Boulogne now possesses two mortifying memorials of the failure of the celebrated preparations made by the French, under Buonaparte, to invade England. She long has had the lofty unfinished Column; and lately, M. Hamy has presented to her Museum a medal, now extremely rare, struck by Napoleon, and intended to commemorate the intended expedition. On the obverse is the head of Buonaparte, crowned with laurel, and the inscription "NAPOLEON EMPEREUR," a title then very recently assumed by him. The design on the reverse is emblematical, but, happily for England, was not prophetical,—A powerful man is stifling in his arms a monster—half human, half fish. The inscription is, "DESCENTE EN ANGLETERRE." It was not impossible but that this allusion might have become true; but on the exergue is the following deliberate lie:—"FRAPPÉE A LONDRES EN 1804"!

Stereotype Plates of Iron.

Hr. Ziegler, a printer of Blankenburg, in Brunswick, has printed a Bible from iron stereotype-plates. The advantages of using this material for such a purpose are not stated.

Improvements in Steam-Carriages on Common Roads.

We noticed in the preceding volume of this Magazine two inventions of M. Galy-Cazalat, which were designed for the improvement of Steam-Carriages. We have since learnt, by a communication from the inventor, that they are part only of a series which has for its object the accomplishment of a problem in which so many have failed, and so much capital has been unproductively expended—the construction of a safe Steam-Carriage, for the conveyance of passengers at a desirable velocity on common roads, which shall be perfectly safe from accidents by explosion, &c.

After a long and careful examination of the subject, and many experiments, on a full scale, M. Galy-Cazalat decided, that the following ameliorations were all desirable in the most improved carriages yet known, and most of them necessary; thus he conceives he has perfectly accomplished in his Steam-Carriage,

1. An arrangement by which the liability of the axle-tree-crank to break is diminished.
2. A mode of suspension of the engine, &c., which prevents its action from being disturbed by joltage[*].
3. An apparatus for guiding the carriage, by means of the steam itself, with great facility.
4. An hydraulic break for diminishing the velocity, and, when desirable, en-

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[*] With a very little more attention, and perhaps no more expense, these morning notices might contain a much greater quantity of information; by this the regular march of the business of the Association might be considerably assisted, and the members enabled greatly to economize their time, during *the little week of four days.* The loss of Monday, and, more particularly, that of Saturday, is a subject of great regret to those who come to work. At Bonn, last year, some of the sections of the German Association, who had interesting matter before them, continued sitting *in the succeeding week, until they had deliberately discharged the whole of their duty.*

[*] Examined and approved by the Institute of France, and rewarded with their gold medal, in 1833.

tirely stopping a steam-carriage, upon a declivity.

5. A steam-generator, of simple construction and little weight; with a fire-place in which coal may be used as a fuel without giving out smoke.

6. An apparatus of great simplicity and of easy application, by which explosions of steam-generators and boilers may be, at all times, *prevented*[*].

7. An apparatus, also of great simplicity, and incapable of derangement, by which the water-surface in steam-generators and boilers is constantly maintained during the working of the engine at the same level[†].

It will be evident to all who understand the subject that, supposing M. Galy-Cazalat has succeeded to the extent which he describes, he has removed nearly all the more important impediments which have up to this moment obstructed the progress of this valuable application of steam-power.

To prevent Ink becoming Mouldy

Add to each pint-bottle of common writing-ink five drops of *kreosote:* it gives the ink a slight odour of smoked meat, which is by no means disagreeable, and effectively obviates its tendency to become musty. The same preventive applies with equal efficacy to Stephens's blue writing-fluid.

Kreosote is a liquid extracted, by a circuitous process, from wood-tar, and may be purchased at the chemists' shops.

Patent-Law Improvement.

The improvement of the Patent-Law is now before the British legislature, and that of the United States. We have carefully prepared abstracts of the several Bills, and we lay them before our readers for comparison. It will be an act of self-mortification for them as it has been for us. They will find, that though the American bill is far from perfect, and is disfigured by some paltry distinctions, yet that it has the merit of attempting to embrace the whole subject, making a "clear stage and showing no favour" to any old and barbarous and unjust and injurious forms, and promulgating a rational and intelligible system of practice, which requires none of the absurd permission nor costly assistance of chief-justices, chief-barons, attornies and solicitors-general, advising barristers, law-agents, &c. &c. Means of information,—of advice,—of assistance,—of wholesome check, are provided for those who are in that pleasing but dangerous delirium, which leads a man to the vestibule of the patent-office, and to the sanctum of a patent-agent. The government and lawyers and agents will not be permitted to seize upon the poor man, who, cursed with inventive genius, makes a step in advance in the march of improvement, and asks for protection from the pirates who surround him. The United States government, by this Bill, is compelled to be ever ready to aid, and the lawyers must stand aloof, at least until the patent is obtained, or refused for reasons given.

The British Patents-Law Amendment Bill, with all the good intentions of its promoters, and much is due to them for their benevolent designs, is a vexatious imperfection compared with the American. The one is a noble statue, sound, detached, complete, somewhat deficient in symmetry and polish. The other, is a mutilated and discoloured trunk, sticking in the niche of an old building, on which some modern artist has been endeavouring, with great difficulty and little success, to attach here and there a new limb, and in other parts to scratch off a little of the dirt with which time, and neglect, and mischievous hands have incrusted it.

Since our last the British Bill has been twice in the "dropped orders" of the House of Commons, and has not yet (26th July) been read a second time. This now stands for the 27th.

Mr. Mackinnon, apparently untired by these repeated delays, has stated in the House that he hopes still to get the Bill into law in this session. We wish his exertions may be successful, for though we feel that the object attempted is hardly deserving of such efforts, still if attained, it may so clear the ground that something better may be easier attained.

British Letters-Patent Amendment Bill—Abstract.

6 Will. IV.—Bill to amend the law relating to Letters-Patent for Inventions,

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[*] Examined, tested, and approved by *La Société d'Encouragement* of Paris, and rewarded with their large gold medal, in December, 1835. Described in p. 395 of *the preceding volume of this* Magazine.

[†] *Described p. 397, as above.*

and for the better Encouragement of the Arts and Manufactures.—Prepared and brought in by Mr. Mackinnon and Mr. Hardy.—Ordered by the House of Commons to be printed, June 20, 1836.

1. First Preamble.—"Whereas it is expedient to alter and amend the law relating to letters-patent for inventions, as well as by rendering more easy and less expensive the manner of securing to individuals the benefit of their inventions, as by affording additional facilities to patentees for the protection of their rights."—Acts entirely repealed 27 Geo. III. c. 38;—29 Geo. III. c. 19;—34 Geo. IV. c. 33;—Act partly repealed, 5 and 6 Will. IV. c. 83;—so far as relates to the notice of objections to be given by defendant to plaintiff on pleading to an action for infringement. Things done, or in progress, in consequence of above Acts not to be affected*.

2. Separate letters-patent for England, Scotland, and Ireland no longer necessary. Letters-patent obtained in one kingdom may be obtained for the other two, on enrolling a specification within six calendar months, and paying the same fees as at present. Letters-patent to be procured in manner in use in England.

3. Total amount of stamps on letters-patent to be 2*l*†.

4. Warrant of a chief-justice, or the chief-baron, directing the attorney and solicitor-general to prepare a bill substituted for the King's signature.

5. A chief-justice, or the chief-baron, to be applied to, to affix his signature to the bill.

6. Such signature to be as valid as the King's. Not to be held unnecessary to obtain the royal signature when the same can be readily obtained.

7. The grant of letters-patent is to bear date from the day of presenting petition. Specification to be enrolled within nine calendar months from presentation of petition. The term for the sole using, &c., of the invention to bear date from the day of sealing.

8. Shops, &c., of persons suspected of infringement, may be searched under a judge's order.

9. Under-sheriff may be directed to attend search.

10. Expenses of search to be defrayed by party applying. In case of verdict against the suspected party, judge may direct him to pay them.

11. On favourable report of Privy Council, the King may grant prolongation of letters-patent for a term, not exceeding fourteen years from expiration of first term. Application for such prolongation to be made with effect before the expiration of the first term.

12. In case of action, notice of objections to be given by defendant and plaintiff as the case may be, and none other allowed to be made at trial. Judge may give leave for others to be offered.

13. Second Preamble.—"That it is expedient for the greater encouragement of the useful arts and manufactures in these realms, to afford some further protection and assistance to the inventors of new and useful improvements, by vesting the property therein in the inventors or proprietors thereof for a limited time." Inventions, by which, in the opinion of the inventor or proprietor, "some new and beneficial operation or result shall be obtained in any art, science, manufacture, or calling whatsoever" secured to the inventor or proprietor for a year from the time of registering. Inventor or proprietor must deposit specimen or model of invention, with his name and address, and pay a sum of money.

14. Three commissioners may be appointed by the King to carry Act into effect, and removed by him on sufficient reason.

15. Commissioners shall provide place for reception of specimens, &c., to be deposited, and preserve and expose them to public inspection for a year from date of deposit. Ten pounds to be paid on deposit of specimen, &c. Commissioners to grant certificates of deposit on payment of one shilling.

16. Persons using, vending, imitating, &c., the subject-matter of a licence, or counterfeiting a certified person's mark on the same, shall be liable to a penalty of 50*l*. Things which have been licensed, may be stamped "licensed," &c., by any person after the term of the license has expired. Holder of license not to be exempted by it from any liabilities in consequence of any infringement of letters-patent. Subject matter of license cannot afterwards be patented, nor licensed a second time.

17. Commissioners to apply monies

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* Though repealed in this Section, it is re-enacted in Section 12 of this Bill.

† The present amount *of stamps for an English Patent is £40., for the three kingdoms about £90.*

received under Act to the payment of expenses of preserving and exhibiting specimens, &c., and account for surplus.

United States Letters-Patent Bill.—Abstract.

BILL to promote the Progress of the Useful Arts, and to repeal all Acts, and parts of Acts, heretofore made for that purpose.—Drawn by a Select Committee of Congress, appointed to take into consideration the state and condition of the Patent-Office, and the Laws relating to the Issue of Patents for new and useful Inventions and Discoveries.

1. New Patent-Office to be established, and attached to the Department of State; chief officer to be called The Commissioner of Patents. Commissioner to superintend the issue of all patents, and have charge of records, models, &c. belonging to Patent-Office;—to have the same salary as Commissioner of Indian Department.

2. To be a Chief Clerk, with salary of £380; two other Clerks, £280 each; Examiner, £340; Draughtsman, £270; Mechanist, £225; and Messenger, £112.

3. All officers to make oath to execute their duties faithfully. Commissioner and Chief Clerk to give bonds and securities for £ each to Treasurer of United States;—to give account to him, quarterly, of all monies received.

4. Commissioner to provide a seal. All records, papers, &c. which have the signature of the Commissioner, and the said seal, shall be evidence. Copies of written documents may be had on paying (about) 5*d.* for every 100 words, and copies of drawings on payment of reasonable expenses.

5. Patents to be issued in the name of The United States, under seal of Patent-Office, signed by Secretary of State, and countersigned by Commissioner of Patents, to be recorded, with the specimens, in said office, &c. Patent to contain short description of invention, and a grant to the inventor and his heirs, of the sole use, &c. for a term not exceeding fourteen years, with a reference to specification for particulars.

6. Inventor to apply to Commissioner, who may grant patent. Inventor first to deliver a written description, drawings, specimens, models, &c. (as the case may be,) so that a person skilled in the art or science to which it appertains, or with which it is most nearly connected, may be able to make the same. Claim to be particularly specified. Documents to be signed by inventor and two witnesses, and filed in Patent-Office. Inventor to make oath, that he believes himself to be the original inventor.

7. Commissioner to cause invention to be examined, and if satisfied it is new and useful, ought to issue a patent. If Commissioner be dissatisfied, or if he think the description, &c. insufficient, to give inventor notice of same, with information for his guidance. Inventor, on withdrawing his application, may receive back one part of duty paid; on persisting in his claim, must make fresh oath. If Commissioner be still dissatisfied, he may have three examiners appointed by Secretary of State, selected for their knowledge and skill in the particular subject. Examiners to be furnished with decision and reasons of the Commissioner in writing. Examiners to give notice to inventor and Commissioner of time and place of meeting, that the latter may give evidence. Examiners may, after inquiry and deliberation, revise the decision of Commissioner, either in whole or part, and Commissioner shall be governed by decision of examiners in the further proceedings. Inventor must deposit with Commissioner, before the examiners are appointed, the sum of £ , which shall be a full compensation for their services.

8. Application for a patent which would interfere with other applications or patents, to be notified by Commissioner to other applicants and patentees. Commissioner may decide upon the priority or right. Case of dissatisfaction of parties to be dealt with as in Section 7.

9. Before consideration by Commissioner of application, inventor must pay, if he be citizen of the United States, or if alien and resident in the United States for a year preceding, having made oath of his intention to become citizen, £9; if subject of the king of Great Britain, £112*; all others, £67 10*s.* Monies received under act to constitute

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* On the principle that an American citizen must pay about the same sum for a patent in England; but it should have been remembered, that an American pays no more here than a native, and this would be the liberal and just rule for the United States legislators.

a fund for the salaries, &c. of the Patent-Office, to be called the Patent-Fund.

10. Heirs of an inventor who may die before a patent, can be obtained, may apply and obtain patent on their making proper oath.

11. Patent, or any part, may be assigned by instrument in writing. Assignment to be recorded in Patent-Office within three months. Assignee to pay £   for the record.

12. Caveat for time to mature invention may be granted. In application for patent, invention to be specified. Such specification of the invention to be preserved secret in Patent-Office. Caveat to be in force for a year; during such time all applications for patents which may interfere, to be notified by Commissioner to holder of caveat. Cases of interfering applications to be dealt with as interfering patents, Sect. 8. Decision of examiners not to preclude the right of contesting the priority of invention in a court having jurisdiction.

13. Patent which may be invalid through insufficient description, or improper claim, without fraudulent intention, may be surrendered to Commissioner, who, on the payment of £3 10*s.* may issue a new Patent for the residue by Patentee of term. Such re-issued Patent to be valid.

Original Patentee may, on payment of £3 10*s.* add description of any new improvement of the original invention, subsequently made. Commissioner to certify time of addition on such added description. Such addition to be then valid as original.

14. Court, on a verdict for plaintiff in an action for infringement, may grant costs not exceeding three times the amount of damages given.

15. Defendant may plead the general issue, and give this Act in evidence, and any special matter of which notice may have been given to the plaintiff thirty days before trial, which may tend to prove that the specification is fraudulently insufficient or redundant, or that the Patentee was not the original inventor, or that the thing patented had been in use or described in some public work anterior to discovery by Patentee; or that it had been publicly used and sold with consent of Patentee before his application, or that Patentee had unjustly obtained a Patent for that which had been *invented by another; or that the Patentee, if an alien at the time the* Patent issued, had neglected for the space of eighteen months from the date of the Patent, to put it in operation in the United States, or to put the invention on sale to the citizens thereof, on reasonable terms; or that if for any period of eighteen months the said invention shall not have been put in operation or on sale, judgment for the defendant in all the preceding cases shall carry costs. Plaintiff to be awarded costs if defendant has used any part of invention justly claimed as new, even if the plaintiff may have claimed more than he invented.

16. In case of interfering Patents, or refusal of Examiners to grant Patent likely, in their opinion, to interfere, the parties interested may have remedy by Bill in Equity. Court may adjudge Patents void, in whole or part, and decide that applicant is entitled to Patent. Commissioner, on such adjudication, may issue Patent. No such judgment to affect the rights of any but parties and those deriving title from them subsequent to judgment.

17. Certain Courts may grant injunctions. Appeal from all may be made to The Supreme Court of the United States.

18. Library of Scientific Works, and Periodical Publications, Foreign and American, to be purchased for Patent-Office. The sum of £   to be appropriated annually for this purpose out of the Patent-Fund.

19. Commissioner to cause models, specimens, &c., patented and unpatented, to be arranged and displayed in proper rooms. Such rooms to be open to public inspection during suitable hours.

20. All previous Acts repealed. Legal proceedings in progress to proceed as though this Act had not passed, except the application of the provisions of the Fourteenth and Fifteenth sections, as far as they may be applicable.

Total Amount, and Annual Average, of Patents in the United States.

The first Act of Congress on the subject of patents was passed in 1790. The average number issued annually from 1790 to 1800 was but 26; from 1800 to 1810, the average number was 91; from 1810 to 1820, it was 200; and, for the last ten years, the average number has been 535. During the last year, there were issued 776; and there have been granted in the first quarter of the present year 274, being more in three months than were issued in the whole of the first period of ten years. In the twenty-two years preceding the war of 1812, the average annual number was 73. The first quarter of the present year indicates an aggregate for the year of 1096; the amount of the duties on which will be upwards of 32,000 dollars (£7200). The whole number issued at the Patent-Office, under the laws of The United States, up to the 31st of March last, is 9731. This is more than double the number which have been issued either in England or France during the same period.—*Report of Select Committee of Congress. United States*, 1836.

High Electrical State of the Atmosphere.

"ONE of the most remarkable meteorological phenomena in this country (New Harmony, Indiana, United States, latitude 38° 11′ N., longitude 87° 55′ W.) is the surprising electrical state of the atmosphere and all non-conducting bodes, during the Autumn and first Winter months, when the wind blows from the north-west. I have observed, at such times, the hairs of the horses' tails so charged, that whole bunches would adhere with considerable force to the horses' flanks; or at other times, the individual hairs, repelled by their neighbours, would stand several inches apart. Distinct shocks may then be obtained in the fingers by rubbing the back of a cat with one hand, and holding the point of its tail firmly with the other. One day last Winter (1834-5), I observed a silk apron attracted by the table so strongly, and at such a distance from it, as to enable the wearer to keep it suspended in the air at a full right angle. When silk, or flannel, is removed rapidly from the body in the dark, it will exhibit for the moment, one luminous electric sheet, and a distinct crackling noise may be heard. Besides these facts, many very interesting phenomena may be observed."—D. D. Owen, *Meteorological Notices on Indiana*, dated New Harmony, 28th May, 1835.

In corroboration of the above, we can *state from personal* remark, that, at *the same place, during* 20¾ successive days in the Autumn of 1826, thunder, near or remote, was heard unceasingly; so that it might be asserted, without exaggeration, that one peal of thunder lasted there for nearly 500 hours!

Railroad Acts, present Session,
(July 23rd, incl.)

THE following additional Railroad Bills received the royal assent on the 4th of July:

18. Sheffield and Rockingham.
19. Hayle.
20. Manchester and Leeds.
21. North of England.
22. London and Cambridge.
23. Eastern Counties (London and Norwich.)
24. Dundee and Newtyle.
25. North Midland.
26. London Grand Junction.
27. Thames Haven.
28. London and Croydon.
29. Preston and Longridge.

Rail-road from Cairo to Suez.

THE works of the railroad from Cairo to Suez are now in active operation, under the personal superintendence of the engineer who designed it, Mr. Galloway, (created a Bey* by the Pasha Mehemet Ali, and therefore known in Egypt as Galloway-Bey). The rails are sent out from England. The total length of the railroad is 80 and 3-10ths English miles. It is by no means level. The summit, 28 miles from Suez, has an elevation of 952 feet above the terminus at that place.

Patent-Law Grievance. No. V.

THE penalties inflicted on the inventive genius of Britain during the present year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £26,000!

N.B. This sum has been paid in *ready money*, on taking the first steps, and as many of the inventors are poor men (operatives,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

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* The title of "Bey" corresponds nearly to that of "Colonel" with us.

NEW PATENTS. 1836.

ENGLISH.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

JUNE *contd.*

164. John Roberts, Prestolle, *Lanc.*, Calico-printer; for improvements in block printing. June 27.—Dec. 27.

Total, June...32.

JULY.

165. Bennett Woodcroft, Manchester, *Lanc.*, Gent; for improved mode of printing certain colours on calico, and other fabrics. July 2.—Jan. 2.

166. William Wainwright Potts, China and Earthenware manufacturer, William Machine, China and Earthenware manufacturer, and William Bourne, all of Burslem, *Staff.*, Manager; for an improved method or process, whereby impressions, or patterns, in one or more colours, or metallic preparations, are produced and transferred to surfaces of metal, wood, cloth, paper, papier-mache, bone, slate, marble, and other suitable substances prepared, or otherwise not being used or known as earthenware, porcelain, china, glass, or other similar substances. July 2.—Jan. 2.

167. Samuel Meggitt, *Hull*, Master Mariner; for improvements in anchors and in apparatus for fishing such improved anchors, which improvements may be adapted to anchors now in use. July 2.—Jan. 2.

168. Robert Walter Swinburne, South Shields, *Durh.*, Agent; for improvements in the manufacture of plate-glass. July 2.—Jan. 2.

169. John Isaac Hawkins, Hampstead-rd., *Middx.*, Civil-engineer; for improvements in manufacturing iron and steel. July 2.—Jan. 2. *For. Comm.*

170. William Southwood Stocker, Birmingham, *Warw.*, Machinist; for improvements in machinery applicable to the making of nails and other purposes. July 7.—Jan. 7.

171. Matthew Heath, Furnival's Inn, *Lond.*, Esq.; for new mechanical combinations for obtaining power and velocity applicable to the propelling of vessels, raising water, and to machinery of various descriptions. July 7.—Jan. 7. *For. Comm.*

172. Elisha Haydon Collier, City-rd., *Middx.*, (formerly of Boston, *United States*,) Civil-engineer; for improvements in steam-boilers. July 13.—Jan. 13.

173. Miles Berry, Chancery-lane, Holborn, *Middx.*, Mechanical Draftsman; for improvements in machinery for forming staves for barrels, casks, and other purposes. July 13.—Jan. 13. *For. Comm.*

174. Louis Matthias Horliac, (late of Paris,) Haymarket, *Middx.*, Gent.; for improvements in carriages and harness. July 13.—Jan. 13. *For. Comm.*

175. Oliver Bird, Woodchester, *Glouc.*, Clothier, and William Lewis, Stroud, in the said county, Clothier; for improvements in machinery applicable to the dressing of woollen and other cloths requiring such process. July 13.—Jan. 13.

176. John Ericsson, Brook-st., New-rd., *Middx.*, Civil-engineer; for an improved propeller applicable to steam-navigation. July 13.—Jan. 13.

177. William Essex, Chatham, near Manchester, *Lanc.*, Agent; for improvements in machinery for producing rotary motion. July 13.—Jan. 13.

178. Samuel Brown, Boswell-court, Cary-st., *Middx.*, Engineer; for improvements for generating gas, which improvements are also applicable to other useful purposes. July 14.—Jan. 14.

179. Charles Phillips, Chipping, *Oxf.*, Surgeon; for improvements in drawing off beer and other liquors from casks or vessels. July 14.—Jan. 14.

180. John Ericsson, Brook-st., New-rd., *Middx.*, Civil-engineer; for improved machinery to be used in the manufacturing of files. July 20.—Jan. 20.

181. Charles Wheatstone, Conduit-st., *Middx.*, Musical-instrument Manufacturer, and John Green, Soho-sq., *Middx.*, Musical-instrument Manufacturer; for new methods of forming musical instruments, in which continuous sounds are produced from strings, wires, or springs. July 26.--Jan. 26.

182. Charles Brandt, Pimlico, *Middx.*, Gent.; for an improved method of evaporating and cooling fluids. July 26.--Jan. 26.

183. Peter Spence, Henry-st., Commercial-rd., *Middx.*, Chemist; for improvements in the manufacture of prussian-blue, prussiate of potash, and plaster-of-paris. July 26.—Jan. 26.

184. John Hall, New Radford, *Nott.*, Lace-manufacturer; for improvements in certain machinery for facilitating the operation of dressing or getting up or finishing of large pieces of lace-net, as bobbin-net, or twist-net, and warp-net, and tattings. July 26.—Jan. 26.

METEOROLOGICAL JOURNAL FOR JUNE, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer Min.	Thermometer Max.	Daily. Temp.	Solar Var.	Rad.	Clouds. A.M.	Clouds. P.M.	Wind. A.M.	Wind. P.M	Direction of wind A.M.	Direction of wind P.M.	Lunation.	WEATHER, &c.
Wed.	1	30·052	61°	30·026	62°	48°4	57°0	52·7	8°6	44°	9	10	1	2	N.	N.N.E.		Scanty rain; day generally overcast, with *cirrus*
Thurs.	2	29·874	62	29·760	64	48·8	67·5	58·2	18·7	45	9	10	2	2	E.	E.		Much cloud; rain, P.M. [cloud.
Friday,	3	29·748	63	29·709	65	50·4	68·6	59·5	18·2	47	7	5	2	3	S.W.	S.W.		Light showers; *cumulus*, and much driving *scud.*
Satur.	4	29·735	63	29·745	66	54·1	67·5	60·8	13.4	50	8	5	3·4	2	S W.bW.	W.S.W.		Wind very high; flying clouds; rainy evening.
SUN.	5	29·870	63	29·935	63	48·2	61·0	54·6	12·8	44	7	3	3	2	W.	W.	☾	Windy; scanty rain at noon; clear night.
Mon.	6	30·165	63	30·142	64	45·5	66·8	56·2	21·3	42	6	8	3	2	S.W.	S.W.		Much cloud; *cirri* and *cirro-cumuli*; windy.
Tues.	7	30·056	63	29·976	64	53·5	64·5	59·0	11·0	50	9	8	2	2	S.S.W.	S.W.S.		Cloudy and lowering; drizzling rain; overcast from
Wed.	8	29·780	63	29·748	67	52·6	69·6	61·1	17·0	50	7	5	3	2	S.W.	S.W.		Fair; passing *cumuli*; distant *nimbi.* [*scud.*
Thurs.	9	29·886	64	29·950	66	51·9	68·8	60·4	16·9	49	5	5	3·4	3	W.S.W.	S.W.		Strong winds; *cumuli*; *cirro-strati* in flocks, and
Friday,	10	29·950	65	29·982	67	56·2	70·0	63·1	13·8	53	10	10	3·4	2	S.W.	S.W.S.		Ditto; overcast; rain at night. [*scud.*
Satur.	11	29·872	66	29·887	67	57·6	70·9	64·2	13·3	56	7	0	2	4·3	S.S.W.	S.W.bW.		Fine; heavy gale, P.M.; *scud.*
SUN.	12	30·245	65	30·285	66	47·8	66·5	57·2	18·7	43	6	2	3	2	W.S.W.	W.		Chilly; much cloud.
Mon.	13	30·400	65	30·404	68	48·2	74·8	61·5	26·6	45	5	3	2	0	W.S.W.	W.		Very fine; heavy clouds.
Tues.	14	30·370	68	30·315	71	57·8	79·0	68.4	21·2	54	6	0	0	1	S.E.	E.	●	Sultry; clouds; *cirro-cumuli.* [to nine, P.M.
Wed.	15	30·054	68	29·976	74	51·2	79·8	65·5	28·6	49	0	8	1	1	E.S.E.	S.		Light clouds; sultry; lightning to south, from eight
Thurs.	16	30·050	72	30·050	73	60·2	74·0	67·1	13·8	58	7	3	1	0	W.	S.		*Cirro-stratus*; drops of rain.
Friday,	17	30·016	72	29·950	73	53·8	76·5	65·1	22·7	51	8	5	1	1	S.	W.		*Cirro-stratus*; *cum.-stratus*; *nimbi*; distant thunder
Satur.	18	29·950	67	29·852	70	50·7	71·9	61·3	21·2	49	3	7	2	3	W.S.W.	S.W.		Fine; few clouds. [to north.
SUN.	19	29·954	67	29.965	69	51·2	69·0	60·1	17·8	49	2	5	2	3	W.S.W.	W.N.W.		Ditto, A.M.—P.M., squalls with thunder-showers.
Mon.	20	30·108	68	30·138	68	53·1	67·5	60.3	14·4	51	4	5	3	3	W.N.W.	N.W.		Windy; *cumulus* and *cirro-cumulus.*
Tues.	21	30·146	67	30·100	69	50·5	70·2	60·4	19·7	49	9	10	3	2	W.b S.	W.		Overcast, with scanty rain.
Wed.	22	30·050	68	30·035	69	55·9	67·0	61·4	11·1	55	10	7	3	3	W.S.W.	W.S.W.	☽	Rain during night; windy; clear at eleven.
Thurs.	23	29·958	66	29·942	67	57·0	68·5	62·7	11·5	53	10	8	2	3·2	S.W.	S.W.		Drizzling rain.
Friday,	24	29·950	66	29·864	68	55·2	70·0	62·6	14·8	53	10	0	2	4·3	S.W.	S.W.W.		Violent squalls of wind and rain; thunder at 4 P.M.
Satur.	25	30·165	64	30·200	66	51·2	67·8	59·5	16·6	49	7	5	3	2	W. b S.	W.S.W.		*Cumulo-stratus*; a shower at 8 P.M.
SUN.	26	30·390	65	30·415	67	50·7	70·2	60·4	19·5	49	8	5	2	1	W.	W.S.W.		Much cloud; *cirr.-cumulus*, and *cum.-stratus.*
Mon.	27	30·450	67	30·386	68	53·4	74·0	63·7	20·6	50	7	4	1	1	S.W.	S.		Ditto, ditto; *cirr.-cumulus*, in flocks.
Tues.	28	30·215	69	30·230	74	55·0	82·1	68·6	27·1	51	1	5	2	2	S.S.W.	W.N.	○	Fine; warm; wind strong at intervals.
Wed.	29	30·430	70	30·425	71	53·5	71.0	62·2	17·5	50	3	0	0	1	N.E.	N.		Hazy; *cirri*; clear night. [at night, to south.
Thurs.	30	30·435	68	30·350	71	48·0	75·1	61·6	27·1	43	2	6	2	2	E.	E.		A breeze; much *cirrus*; continuous brilliant lightning
Mean		30·0775	66	30·0581	68	52.38	70·24	61·32	17·86									

Bar. Max. 30 in. ·461 on the 26th. ‖ Ther. Max. 82°1 on the 28th. ‖ Lowest point of Rad. 43°, on the 30th.
Bar. Min. 29 in. ·700 3rd. ‖ Ther. Min. 45°5 6th. ‖ Rain fallen 1·070 in.

ON THE STUDY OF MATHEMATICS.

Our readers are aware that attention has been lately recalled to a controversy of long standing, by the appearance of Mr. Whewell's pamphlet *On the Study of Mathematics*, which elicited an article in the *Edinburgh Review*, written with a bitterness seldom excited but by personal resentment, but with that critical acumen and profound learning which has so deservedly rendered that journal a leading one in literature. The *professed* object of the writer is to show, that mathematics, when made a principal study, instead of being calculated to develop and mature the intellectual faculties, have a directly contrary tendency, and train the mind to a "one-sidedness" which disqualifies it for intercourse with others, and for sound philosophical reasoning. Professor Chevallier, of the Durham University, has replied to this attack, with an eloquence which exhibits him as a powerful champion on the other side. We shall endeavour to place the merits of the question in a simple form before our readers, to the end that they may judge for themselves on a point so vitally connected with systematic education; we will strive to be as impartial in weighing the evidence as is compatible with a very decided opinion on the subject; one requisite to candour on the question we at least possess—that of not owing our education to either of the two universities which have been implicated in the discussion; our bias is, therefore, not due to those associations of filial reverence which these *almæ matres* so naturally excite in their children. If in our endeavours to justify mathematical study, and defend it from the attacks of its enemies, we appear exclusively to direct our arguments against the article alluded to, it is because we consider it as embodying, in the most detailed and luminous form, all the objections that have ever been made.

The question at issue has been simplified by excluding all consideration of the *utility* of the sciences brought into contrast during the argument; they have been considered solely "as the means of a liberal education—that is, an education in which the individual is cultivated, not as an instrument towards some ulterior end, but as an end unto himself alone; in other words, in which his absolute perfection as a man, not his relative dexterity as a professional man, is the scope immediately in view." We confess, however, that this distinction appears to us more specious than necessary, every man has an ulterior end in view, though it may not be the acquisition of wealth, or even the means of subsistence; emancipation from these cares not only does not enable the mind to attain universal knowledge, but by removing a powerful incentive to exertion generally renders it less active; it is the opposite tendencies of very general and comprehensive divisions of studies which are to be canvassed, the comparison, as will appear, is virtually instituted between abstract and physical science, and literature in the most comprehensive sense of that term; now the student becomes devoted to one or the other of these divisions from causes quite independent of, and long anterior to the choice of a profession, which choice is greatly *influenced by the inclination* manifested towards one of these *branches of study in preference to* the other, and professional success must subsequently, in a great measure, depend on the general intellectual cultivation of the candidate.

The essential preliminary to such a controversy ought, we conceive, to have been a definition of what constitutes a liberal education, and of the tests by which we may estimate the degree of success in its attainment; we presume the one generally received may be adopted here—which defines the most efficient education to be that which secures the greatest quantity of happiness to the individual, happiness being previously shown to depend on moral conduct, and that this again is fostered by mental discipline as conducing to the subjugation of our passions. If that command over our passions be obtained, it must be comparatively unimportant what may be the subjects with which the intellect is occupied; but since variety must be presented to it in order to keep the mind in constant activity, no one study exclusively pursued can be adequate to the object; all parties, accordingly, insist on diversity of pursuits, as a mean of intellectual cultivation, and the question becomes —Which ought to have the preference as the more engrossing occupation? Surely that which involves the greatest number of others, or that which cannot be successfully prosecuted independently of others.

Without entering into any metaphysical discussions, we may primarily refer the intellectual faculties which it is the object of education to develop, to those of *judgment* and *imagination;* the studies by which the former is chiefly matured are such as exercise the reasoning powers, and "enable us to trace securely and readily the necessary consequences of assumed principles."

"In a great part of mankind such a power requires to be confirmed and strengthened by education, since by nature it exists in a low degree and confused form only; men's minds are full of convictions which they cannot justify by connected reasoning, however reasonable they may be. . . . . . There prevails very widely an obscurity or perplexity of thought, which prevents men from seeing clearly the necessary connexion of their principles with their conclusions.

"Now though there is this chance of being practically right and speculatively wrong, it will probably be allowed that this is not a state of mind in which those can acquiesce contentedly, whose object is the mental culture of man. . . . . . The object of a *liberal education* is to develop the whole mental system of man, and thus to bring it into consistency with itself;—to make his speculative inferences coincide with his practical convictions;—to enable him to render a reason for the belief that is in him, and not to leave him in the condition of Solomon's sluggard, who is wiser in his own conceit than seven men that *can* render a reason.

"This complete mental culture must, no doubt, consist of many elements; but it is certain that an indispensable portion of it is such a discipline of the reasoning power as will enable persons to proceed with certainty and facility from fundamental principles to their consequences. . . . . .

"There are two principal means which have been used for this purpose in our universities;—the study of *mathematics* and the study of *logic*. These may be considered respectively as the teaching of reasoning by practice and by rule. In the former study, the student is rendered familiar with the most perfect examples of strict inference; compelled habitually to fix his attention on those conditions on which the cogency of the demonstration depends; and in the mistaken and imperfect attempts at demonstration made by himself or others, he is presented with examples of the most natural fallacies, which he sees exposed and corrected. In studying logic, on the other hand, a person finds the conditions formally stated under which an inference is legitimate; he is enjoined to see that in any given case these conditions are satisfied; and if a fallacy exists, he is provided with rules by which it may be condemned and made more glaringly wrong. . . . . . .

"Mathematics familiarize the student with the usual forms of inference, till they find a ready passage through his mind, while anything which is fallacious and logically wrong at once shocks his habits of thought, and is rejected. He is accustomed to a chain of deduction, where each link hangs from the preceding; and thus he learns continuity of attention and coherency of thought. His notice is steadily fixed upon those circumstances only in the subject on which the demonstrativeness depends; and thus that mixture of various grounds of conviction, which is so common in other men's minds, is rigorously excluded from his. He knows that all depends upon his first principles, and flows inevitably from them; that however far he may have travelled, he can at will go over any portion of his path, and satisfy himself that it is legitimate; and thus he acquires a just persuasion of the importance of principles, on the one hand, and, on the other, of the necessary and constant identity of the conclusions legitimately deduced from them. Logic, on the contrary, forcing upon our notice the rules which we follow when we reason well, hardly allows them to become so habitual as to escape our consciousness; nor does she familiarize us with long trains of strict reasoning, since she generally gives special deductions only as examples of forms of argument. And thus the continuity and concentration of thought, and the quick sense of demonstration which it is our aim to educe, are not taught so well by this study as by that of mathematics."—(Whewell *on the Study of Mathematics, &c.*, pp. 4—7.)

The progressive enlargement of the boundaries of our knowledge, and consequent multiplication of subjects with which the mind may be occupied, compels every one to make a selection of them for study; it is only in times when science is extremely limited and vague, that universal geniuses can exist, such as a Mirandola or a Crichton are represented to have been; in the present day men are attracted towards that branch of study for which their intellectual constitution more particularly inclines them, and they are obliged to content themselves with a general acquaintance with the others; the comparative value of each as a mean of mental culture, can only be estimated by the average result of that peculiar study on individuals, and if any one study appears steadily to decline in general estimation, it is a presumptive proof that it is found to be either inadequate to mental discipline, or is relinquished from causes over which we have no control. Now we think that this tendency to decreasing estimation has manifested itself for a considerable period, with regard to those studies denominated *philosophy* by the reviewer; on such matters individual must yield to general opinion; it is futile endeavouring to stigmatize the latter by attributing it to the spread of *utilitarian* or *revolutionary* principles; the doctrines thus designated could not extend themselves, unless they were found to contribute ulteriorly to general happiness, or to have become involved in our well-doing.

There can exist no permanent standard with which to compare the several studies that may engage the mind, for the purpose of deciding on their relative merits; as the various relations among the great family of mankind increase in number and complexity, the general plan of intellectual cultivation must be modified, the one essential condition being kept in view—that of controlling the passions and cultivating the social virtues; in order to fulfil this condition, certain studies must always be insisted on as essential to a liberal education; the relations of man to his Creator must obviously be the first of these, and all those that tend to repress our self-sufficiency and presumption, and to increase our charity towards other beings; but the means for inculcating these feelings, and for maturing our conceptions of these relations, may vary, and have varied greatly; the increasing difficulty of providing for our physical, and of gratifying our acquired wants, has compelled us to study with greater attention the qualities of the material universe, and has, comparatively, withdrawn the mind from those reflections on its own operations, which solicited it in times when man had more leisure for speculation, and less necessity for action. Fortunately, this inevitable change in the direction of our pursuits is favourable, as we shall endeavour to show, to the paramount object we have alluded to.

In impugning the efficiency of mathematics as a means of intellectual culture, an arbitrary limit to that science has been assumed, which is not admitted by its advocates, and is grounded, as we think, on an erroneous conception of the distinction between *pure* and mixed mathematics. We shall first comment on the limitation alluded to.

> "In the first place, it is wholly beyond the domain of mathematics to inquire into the origin and nature of their principles. Mathematics, as Plato and Proclus observe, are founded on hypotheses of which they can render no account. The geometer (says Aristotle) can attempt no discussion of his principles; and Seneca observes that *mathematical is a superficial science, it builds on a borrowed site, and the principles, by aid of which it proceeds, are not its own: philosophy begs nothing from another; it rears its own edifice from its own soil.* These authorities represent the harmonious opinion of philosophers and mathematicians of ancient and modern times."—(*Edinb. Review*, No. 126, p. 415.)

Now we would ask any one who does not insist on nice subtilties, whether the processes of the mind, by which we arrive at those abstractions *absolutely necessary* to the study of mathematics in every stage, may not be fairly considered as pertaining to its domains? The ideas of *space*, *quantity*, *ratio*, *limit*, *force*, *motion*, *time*, &c., require the most strenuous intellectual effort to grasp, yet they must be rightly conceived by every mathematician from an early period of his study; surely it is unfair to require that he should do so, and yet deny him the right of reasoning on them? They must certainly be regarded as forming an integral part of that science, if the notions of *fate*, *necessity*, *evil*, *taste*, *beauty*, &c., be considered as belonging to the domain of ethical. It is useless, in a question of practical bearing, to insist on any strict boundary to different sciences, if one study absolutely necessitates "an inroad into the province of another," it must be regarded as an additional recommendation of the former, by every one advocating diversity of intellectual pursuits.

"But as for operating according to rules, and by the help of general forms, whereof the original principles and reasons are not understood, this is to be esteemed merely technical. Be the principles ever so abstract and metaphysical, they must be studied by whoever would comprehend the doctrine of Fluxions. Nor can any geometer have a right to apply the rules of the great author, without first considering the metaphysical notions whence they are derived."—(*The Analyst*, § xlvii.)

The distinction between pure and mixed mathematics consists in this,—the latter, in addition to the abstractions of the pure science, involve the results of experiments and observations on the *qualities* of matter, which, of course, do not enter into the former, but which must be made the basis of our deductions in the mixed science. *Mechanics*, as investigating the simple laws of *motion* deduced from arbitrary hypothesis, is a branch of pure mathematics, though the notions of *cause* and *effect*, of *force* and *inertia* are necessary to the comprehension of those laws; but when we have to take into consideration the effects of *gravity*, *friction*, *elasticity*, *fluidity*, &c., which can only be ascertained by observation, the sciences which embrace these results, such as physical astronomy, hydrodynamics, the theories of sound and light, &c., are mixed, yet they are still essentially mathematical, because the general expressions which embody their laws can only be obtained by pure mathematical investigations.

"Observation upon the phenomena of the natural world soon leads us to the conclusion, that the changes which we observe taking place around us are not fortuitous, but closely connected with some previously-existing conditions. We thus arrive at the notion of the connexion between cause and effect; and whatever metaphysical difficulties there may be in conceiving the nature of such a connexion, its existence is, in many instances, a *fact*, established by constant and invariable experience. Some of the changes thus produced may be of such a nature, that their mutual relation is capable of being expressed by that which one abstract quantity bears to another; and when this is the case the phenomenon is brought within the scope of mathematical reasoning, and constitutes a part of that extensive branch of physical science, denominated mixed or applied mathematics.

"The two branches of mathematical study thus intimately united comprise the whole body of demonstrative science: and I include them both as subjects of our present inquiry, not for the purpose of eluding an accusation against the utility or importance of mathematical study, but because I know of no instance, in which the study of pure mathematics is exclusively pursued, as a part of a liberal education; and because such a view of the question is in strict accordance with the history and progress of mathematical science. As the natural philosopher has accumulated his observations, and recorded his results, he has called in the aid of the geometer and the analyst, to reconcile apparent inconsistency, to give coherence to isolated facts, to ascend by induction from numerous individual examples to some one great originating cause."—(Chevallier *on the Study of Mathematics*, pp. 8, 9.)

It should seem from the tenour of the objections urged against the science, that its opponents wish to confine the term mathematics to those elementary branches, during the attainment of which the mind is only engaged with abstract hypotheses, or with conventional symbols. If the question under discussion were confined to the utility, as an *exclusive study*, of the science thus limited; it would be soon settled by the admission of most of the objections urged against it; and then we know none that would not equally apply to theoretical logic, or to any other speculative science, when made an *exclusive* study. But a knowledge of these elements alone, no more constitutes that of the science, than an acquaintance with the *accidence* of a language entitles a person to consider himself as master of it, without his having acquired its idiom, and being able to speak, or at least to read it; no one capable of carrying his attainments further ever was contented to stop at this outset, or at least ever to regard his acquisitions otherwise than merely as a *means* of obtaining real knowledge if he thought fit to employ them.

It might have been expected that the anti-mathematicians would have been more explicit in stating what are the studies which they intend to contrast with that science, as more favourable to the culture of the intellectual faculties; the assumed term *philosophy* is unmeaning, and little preferable to that of *humanities*, by which these studies are designated in certain universities. We may concisely class all studies under one or other of these heads:—PHYSICS, comprehending every science which has matter or its attributes for its object: ETHICS, taken simply as a general term, to comprise the various phases under which mind and its operations may be contemplated. Physics are naturally divided into *mathematics*, having magnitude and quantity, with the derivative conceptions of motion, time, &c., abstracted from all considerations of matter for its subjects: *chemistry*, by which the mutual actions of inorganic matter are ascertained and illustrated: and *natural history*, including every study which has the organic creation for its subject*.

Mathematics and logic have been considered as having a certain analogy; both are conversant about assumed hypotheses, both may be acquired without reference to the external world, except for illustrations; at all events both are become associated in our minds, by their being made the primary studies at our two universities: there is, however, in addition to many others, this striking difference,—the former can only be acquired by long study and application, and they *must* be acquired, if we intend to arrive at a sound knowledge of physical science†; while on the contrary we acquire the practice of logic intuitively with the progressive exercise of our reason, from the commencement of our intellectual culture, much in the same way as we learn to walk before we know what the "centre of gravity" means, or are convinced of the necessity for bringing it over the basis of our feet if we intend to stand firmly.

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* The candid reader will at once see that this division of sciences is only adopted to simplify our argument, and that we are aware of its insufficiency for any other purpose. For a valuable dissertation on this subject he may refer to the introductory discourse on the progress of Ethical Science, by Mr. Dugald Stewart, prefixed to the Supplement to the *Ency. Britannica.*

† The accurate study of pure mathematics is the only foundation upon which a solid superstructure of natural philosophy can be built. And all attempts effectually to supply the want of sufficient mathematical knowledge by mere popular explanation, or by intentionally couching the expression of physical facts in language which appears to be familiar, while it is only ambiguous, must fail to accomplish their purpose. From the time of Archimedes to the present day, no *royal road* has been discovered into the distant regions of mathematical and physical science. They who would reach the goal must encounter the fatigue: they who would win the crown, must descend into the arena, and gird themselves manfully to the conflict.—(*Chevallier*, p. 15.)

That this is a just view of the practical value of logic as a science, is shown by the general neglect of it by those who have not received an university education; certainly an acquaintance with it may enable us more readily to expose, or, perhaps, avoid false reasoning; yet we are not the less conscious of its existence, although we may never have heard of *contingent and necessary matter*, nor of the eleven moods, and may not be able to point out an *undistributed middle term*, or an *illicit process*. We are far from undervaluing logic as a *science*, or its interest to every one who reflects on the processes of thought; but the world, by its neglect of it, has, in a great measure, decided on its inutility as a guide to our reasoning powers.

This relative estimation of the two studies is further manifested by the increasing proportion of students who go to Cambridge in preference to Oxford: at the end of the last century the numerical superiority was greatly in favour of the last named university, while at the present time Cambridge boasts of an absolute majority of 300 members in 5500, and has received an accession of seventy odd since last year. Let us here adduce a testimony of no mean weight, both as coming from a scholar of Oxford, and from one of the first logicians of the day.

"It was doubtless from a strong and deliberate conviction of the advantages, direct and indirect, accruing from an acquaintance with logic, that the university of Oxford, when remodelling their system, not only retained that branch of study, but even assigned a prominent place to it, by making it an indispensable part of the examination for the first degree. This last circumstance, however, has, I am convinced, produced an effect opposite to what was designed. It has contributed to lower, instead of exalting the estimation of the study . . . . . it might have been, and doubtless was, expected that a majority, at least, of successful candidates would derive some benefit worth mentioning from their logical pursuits; and that a considerable proportion of the distinguished candidates would prove respectable if not eminent logicians. Experience has shown that these expectations have been very inadequately realized. The truth is that a very small proportion even of distinguished students, ever become proficients in logic, and that by far the greater part pass through the university without knowing anything at all of the subject. I do not mean that they have not learned by rote a string of technical terms; but that they understand absolutely nothing whatever of the principles of the science. . . . . . . Permission is granted to such as are candidates merely for a testimonial, to substitute for logic a portion of Euclid. I fear, however, that little or nothing will be gained by this; unless indeed the examiners resolve to make the examinations in logic far stricter than those in Euclid. For since every one who is really capable of understanding Euclid must be also capable of logic, the alteration does not meet the case of those whose inaptitude for science is invincible,—those who are physically incapable of scientific reasoning, and the far greater number who fancy themselves so,—all these will be likely, when the alternative is proposed, to prefer logic to Euclid; because in the latter it is hardly possible, as in logic, to present the semblance of preparation by learning questions and answers by rote, in the cant phrase of undergraduates,—by getting *crammed*. . . . . . those most averse to science, or incapable of it, are almost always found to prefer logic." —(Archbishop Whately's Preface to his *Elements of Logic*.)

"In practice, logic is more a trick than a science, formed rather to amuse than instruct. And in some sort we may apply to the art of syllogism what a man of wit (Butler) observed of rhetoric, '*that it only tells us how to name those tools which nature had before put into our hands, and habit taught the use of.*'"—(Warburton, Preface to *Julian.*)

These avowals, combined with the fact of the increasing popularity of the Cambridge system of study, form, we think, a fair ground for presuming that "the revolutionary tendency in popular opinions, regarding the objects, and the end of education, which, in this nation at least, is becoming daily more and more obtrusive,—the extended study of mathematics being that mainly proposed in lieu of the ancient branches of discipline, which our innovators would retrench," owes its rise to an increasing perception of the relative merits of the two*.

The reviewer in support of his opinions cites those of several celebrated persons, tending to prove the narrow, or "one-sided" kind of intellectual culture received from mathematical study; it is remarkable, after his careful researches, how little satisfactory evidence he has been able to produce. After stating his intentions of adducing the opinions of mathematicians themselves, of four from whose works passages are selected, D'Alembert and Pascal are the only ones of mathematical celebrity; the others being Berkeley and S'Gravesande. Let us briefly examine what is the substance of their allegations.

"It seems as if great mathematicians ought to be excellent metaphysicians, at least upon the objects about which their own science is conversant; nevertheless this is very far from being *always* the case. The logic of *some* of them is comprehended in their formulæ, and does not extend beyond. The case resembles that of a man who has the sense of sight contrary to that of touch, or in whom the latter of these senses is only perfected at the expense of the former. These bad metaphysicians in a science in which it is so easy not to reason wrong, would infallibly be much worse, as experience proves, on matters in which they had not the calculus for a guide."—(D'Alembert's *Elemèns de Philosophie*, as quoted in the *Edinburgh Review.*)

The authority of such a writer as D'Alembert would have been too valuable to allow an active partisan to spare any pains in obtaining his favourable evidence; we congratulate the mathematicians that nothing more can be wrung from him than that there are *some* indifferent metaphysicians among mathematicians; and that if they reason wrongly in a science in which it is difficult to do so, they would reason much worse on other matters.

It is clear from the expressions of the other witnesses, that they took the same limited view of the science which the critic himself does. Thus Berkeley asks, "whether tedious calculations in algebra and fluxions be the likeliest method to improve the mind; and whether men's being accustomed to reason altogether about mathematical signs and figures doth not make them at a loss how to reason without them." We would ask of any one conversant with more than the solution of a simple equation, whether the train of reasoning by which the mind arrives at right notions of limiting ratios, of imaginary quantities, and of the relative advantages of employing the idea of motion, or the doctrine of infinitesimals, in explanation of the higher analysis, can be considered as nothing but reasoning about *signs* and *figures*.

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* How is the reviewer justified in asserting that the "university of Cambridge restricts to the narrowest (mathematical) proficiency, all places of distinction and emolument to which such honours constitute a claim;—thus also leaving the immense majority of its alumni without incitement, and the most arduous and important studies without encouragement and reward?"

There are a number of prizes of books, medals, money, amounting in value to about 1300*l.* annually distributed as incentives to exertion, *three-fourths* being given for classical attainments and English composition, and the remaining fourth for mathematical skill. The colleges distribute in addition about 600*l.* more in the same form among their scholars; *two-thirds*, we believe, of which is given to encourage classical literature. It is not the value, but the *proportion* of the distribution to which we call attention.

Berkeley himself, in the same work, (the *Analyst*,) among his queries proposes also the following: "Whether ever since the recovery of mathematical learning, there have not been perpetual disputes and controversies among mathematicians? and whether this does not disparage the evidence of their methods?" Is not this an admission that something more than mere necessary truths and conventional symbols enter into mathematics? for, according to the essential character of these, they can admit of no dispute.

> "How, in a science to which the merit of exactitude is generally attributed, can it arise, that men of the highest talent are divided in opinion? The reason is, that in problems, the solution of which cannot be submitted to conclusive experiment, besides the part of calculation, subjected to rigorous laws, and on which there cannot be two opinions, there exists another, a *metaphysical* part, which is productive of doubt and obscurity."—(*Memoir on La Grange, by Delambre.*)

The reader will be surprised when he learns who are the other authors whose testimonies against mathematics are quoted with exultation in the Review. Sir Kenelm Digby, Sorbière, Clarendon, Le Clerc, Budæus, Barbeyrac, Basedow, Walpole, Warburton, Gibbon, Kirwan, and Madame de Staël, form the phalanx; it is not, of course, by the celebrity or obscurity of the writers that the value of their arguments are to be measured, but we object, in a great measure, to any individual *opinion* on such a subject, when we take into consideration the natural bias we all feel to underrate those acquirements we do not ourselves possess; we should listen with equal suspicion to an encomium on mathematics from Newton, Euler, D'Alembert, or La Place, as to any strictures on them from philologists, jurists, or poets; accordingly, when we examine the evidence brought into court by our opponent, we find that it either owes its colouring to this principle, or that it merely represents the inefficiency of any one study fully to develop all the intellectual faculties,—a proposition concerning which there was no dispute.

Sir K. Digby says, "It hath been generally noted that the exactest mathematicians have seldom proved eminent in metaphysics or speculative divinity; nor, again, the professors of these sciences in the other arts;" which is merely saying that it is not common for the same individual to *excel* in two arduous studies. Clarendon is cited as mentioning that the Earl of Leicester was addicted to the mathematics, and that they failed to make him either a good soldier or a profound politician! We much question whether the noble earl was even a decent arithmetician in addition and subtraction. Le Clerc sagaciously observes that *if* mathematicians "apply the rules of their science to judge of the administration of public or private affairs, they would arrive at the most absurd conclusions." What useful deduction, bearing on the argument, can be made by the reviewer, from the fact that Gibbon was averse to mathematical study, and abandoned it at an early period, to follow those pursuits for which he felt a more decided vocation? Surely this "multiplying of arguments, especially frivolous ones, such as are all that are merely verbal, is not only lost labour, but cumbers the memory to no purpose, and serves only to hinder it from seizing and holding the truth*."

This vocation, this bias of the mind towards any one line of study, is the result, we may conclude, of the greater energy of that class of faculties which are most exerted in those studies; whenever this bias is thwarted, results may be expected unfavourable to the full development of the intellect; and is it not probable that most of those irregularities, or eccentricities, which manifest themselves in the minds of men, are the consequences of this violence? We have already hinted that the intellectual, like the bodily, constitution, is in a great measure formed anteriorly to any artificial culture by systematic education; when, owing to favourable circumstances, the individual is enabled to follow the "bent of his genius," as it is popularly termed, though he may *excel* in one pursuit only, yet, from having undergone no prejudicial check, his mind exhibits all its faculties in a healthy tone of proportional vigour.

The poet by nature, who from childhood yields to the influence of his imagination, and produces at maturity the "thoughts that breathe, the words that burn," often manifests a sound judgment on all topics; but if that intellect, in which imagination was, from its origin, predominant, had been forced from its natural bias, and the born-poet had been trained to be a mathematician or a jurist, might it not be anticipated that a one-sidedness of mind would be the result, which ought not to be attributed to the adopted study, *quoad* study, but to the violence, done to the faculties? On the other hand, when that attraction towards mathematical reasoning is felt, which induces the pursuit of the science through life, unimpeded by mistaken interference, an overwhelming number of instances can be adduced to show that the mind of the mathematician is not one-sided, that he manifests genius in his favourite pursuit, and that all his faculties are sound and vigorous, though in an inferior degree to those, whatever they may be, which are more particularly mathematical†.

Let us now proceed to examine the justice of the charge brought against mathematics, in the following quotation from Warburton, as given in the Review, and we copy it entire, because the special pleading throughout the article is but an amplification of this eloquent passage.

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* Locke, *Conduct of the Understanding*, sec. 16.

† The mathematical reader will recall to his mind the purely metaphysical reasoning of numberless writers on the subject; among many others, the able work of the late R. Woodhouse, on the *Principles of Analytical Calculation*, would alone convince any one that profound knowledge of mathematics was not incompatible with profound reasoning on other topics. The series of works lately published, called the *Bridgewater Treatises*, also afford a pregnant illustration; the reasoning of the mathematical writers in those volumes is nowise inferior to that of the naturalists and divines, to say the least of it. Dr. Turton, by his recent work on *Natural Theology*, has shown that mathematical knowledge of the highest kind does not impede the attainment of critical and metaphysical learning rarely equalled; while, to the varied and extensive knowledge of Mr. Whewell, the *Reviewer* bears a generous and ample testimony.

> "It may seem, perhaps, too much a paradox to say, that long habit in this science incapacitates the mind for reasoning at large, and especially in the search for moral truth. And yet, I believe, nothing is more certain. The object of geometry is demonstration, and its subject admits of it, and is almost the only one that doth. In this science, whatever is not demonstration is nothing, or at least below the sublime inquirer's regard. *Probability*, through its almost infinite degrees from simple ignorance up to absolute certainty, is the *terra incognita* of the geometrician. And yet here it is that the great business of the human mind is carried on;—the search and discovery of all the important truths which concern us as reasonable creatures. And here, too, it is that all its vigour is exerted; for to proportion the assent to the probability accompanying every varying degree of moral evidence, requires the most enlarged and sovereign exercise of reason. But the harder the use of anything, the more of habit is required to make us perfect in it. Is it then likely that the *geometer*, long confined to the routine of demonstration, the easiest exercise of reason, where much less of the *vigour* than the attention of mind is required to excel, should form a right judgment on subjects, whose truth or falsehood is to be rated by the probabilities of moral evidence?"—(Preface to *Julian*.)

We have already protested against the arbitrary limitation of mathematics to those elementary branches, during the study of which the mind is only engaged with *necessary* truths, and conventional symbols; we should not, however, conceive ourselves entitled to extend its boundaries indefinitely, if we could not show that its cultivation *inevitably* leads to that of other sciences. If the philologist, in tracing the origin of languages, is led to study the political and social history of nations, and must be conversant with many other branches of ethical study; if the metaphysician and moral philosopher cannot pursue their inquiries without some knowledge of physiology; so the mathematician must become acquainted with most branches of physics in the prosecution of his pursuits. Mathematics do not owe their existence to the necessity for intellectual cultivation, but were cultivated because they ultimately enabled men to measure, and weigh matter in its absolute reality; the meaning of the term *geometry* reminds us of its practical origin and bearing; it is a strange fallacy, therefore, that treats the speculative science as complete in itself, and independent of the material world. The mathematician becomes acquainted with most branches of physical science, because he cannot well master the speculative part without constantly applying his deductions to real or assumed cases. If geometry be imagined to owe its origin to the necessity for restoring land-marks, effaced by the overflowing of the Nile, it is notorious that the higher analysis owes its maturity, if not its existence, to the inadequacy of geometry to investigate the laws of planetary motion.

The mathematician feels that the definitions on which his deductions are based have no existence, but are pure abstractions of his own mind; and only constitute a scaffolding, a frame-work, on which he must combine into comprehensive theorems the isolated facts to be derived from the material world around him. Accordingly he strives onward till he reaches the palpable,—till he can apply his abstractions to what passes independent of him. The metaphysician, on the contrary, cannot travel out of his own mind, he has nothing extrinsic with which he can test the justice of his conclusions, his is the fruitless endeavour of the eye striving to contemplate itself without something to reflect its image.

"Mixed mathematics are primarily dependent on the extent and accuracy of our scrutiny into nature, after which their further advancement is limited only by the degree of perfection to which the pure are carried. But to a very moderate progress in the former a very perfect knowledge of the latter is requisite. The laws of nature are, for the most part, simple in themselves, but the circumstances under which they act, induce a complication in their agencies which calls at once for the most powerful exertions of natural reason, and the most refined artifices of practised ingenuity to develop. Combinations are perpetually presenting themselves when the principles are satisfactorily known,—the general laws placed beyond a doubt,—the mode of applying mathematical investigation thoroughly understood,—yet which, by the mere complication of the pure mathematical inquiries they involve, defy the utmost powers of calculation. The restless activity of Nature surrounds us with minute phenomena of this kind; the motions and equilibrium of fluids, their capillary attractions, the vibrations of the atmosphere of solid bodies, every breath of wind that blows, every mote that sparkles in the sunbeam, supplies us with an instance in point. On a wider scale, the law of gravitation, modified by the consideration of three gravitating bodies in motion, produces a problem which has resisted every effort of ingenuity and industry, stimulated by the strongest motives which can excite men to exertion.—(Sir John Herschel, art. MATHEMATICS in *Brewster's Encyclopædia.*)

It is to celebrated mathematicians accordingly that we are principally indebted for our extended knowledge of the material world. Young, Herschel, Arago, Fresnel, and many others whom we could name, have, by maturing the theory of light alone, done as much towards enlarging our minds, and instilling new and imposing ideas, as was ever accomplished by the metaphysician in his endeavours to illustrate our mental operations; that mind must indeed be obscured and prejudiced by the scholastic discipline of exclusive universities, which can underrate the moral effect of the recent researches on heat, magnetism, &c., as instituted by Leslie, Barlow, Melloni, &c., in short, there is not a modern discovery in physics which we do not owe to a mind necessarily trained in mathematical reasoning. The characteristic of mathematical reasoning,—that of being a successive chain of deductions from assumed principles, forces itself so constantly, and so obtrusively on the mind, that we confidently assert, mathematicians, better than most others, can appreciate and understand the force of moral evidence and probability; since it must be allowed that he who can institute a comparison between two species of conceptions, is more likely to rectify both, than he who has only one present to his mind. The learner, when for the first time he comprehends the demonstration of a problem in Euclid, attains a conception of the *limit*, to use a mathematical phrase, to the "infinite chain of probability," and can appreciate the force of any link, by at once perceiving its relation to that limit. The politician, or the man of the world, who knows no *other than moral probability* as a ground for conviction, is more liable to appreciate at an equal value different evidence of this nature, unless he is convinced, like the geometrician, that there are "empirical facts" and "necessary matter."

"The study of mathematics would show them the necessity there is in reasoning, to separate all the distinct ideas, and see the habitudes that all those concerned in the present inquiry have to one another, and to lay by those which relate not to the proposition in hand, and wholly leave them out of the reckoning. This is that which in other subjects besides quantity, is what is absolutely requisite to just reasoning, though in them it is not so easily observed nor so carefully practised."—(*Conduct of the Understanding*, sec. 7.)

We have now to consider the charge made against mathematical study, "that it disposes the mind to one of two opposite extremes, *credulity* and *scepticism*."

"Alienated by the opposite character of their studies from those habits of caution and confidence, of skill and sagacity, which the pursuit of knowledge in the universe of probability requires and induces, they (mathematicians) are constrained, when they venture to speculate beyond their diagrams and calculations, on the one hand to accept their facts, either on authority or on imagination; or, on the other, to repudiate altogether as unreal what they are themselves incapable of verifying."—(*Review*, p. 441.)

To substantiate this view the writer quotes this passage from a living German metaphysician, (J. Sulat). "In so far as the mathematician is accustomed to his own mode of thinking, and ignorant of any other, applies, or does not apply, it to the supersensible, what must follow? In the former case the supersensible world is denied, inasmuch as it cannot be mathematically demonstrated; and in the latter, affirmed only on the ground of feeling and imagination. Thus on the one alternative, the mathematician becomes necessarily a *materialist;* on the other, a *mystic*."

It is impossible to avoid the semblance of repetition in an endeavour to confute a series of assumptions based on the same fallacy. Having denied the confined nature of mathematical pursuits, and having shown that they necessarily imply an acquaintance with, and study of, the phenomena of the material world, there exists no *à priori* reason for assuming that they have that prejudicial effect on the mental faculties which it has been asserted they have, by those who took an erroneous or partial view of their domains. This science has no peculiar tendency to produce that defect in the faculty of judgment which is implied by *credulity*, since its cultivation does not, necessarily, exclude that of other modes of reasoning, or other intellectual pursuits, by which the judgment may be strengthened and habituated to the exercise of caution, skill, and sagacity. If by *scepticism* is meant only the suspension of the judgment till more evidence is brought forward by which it may be formed, this cannot be surely imputed as a defect in the reasoning faculty; and we trust that no other meaning was covertly intended,—that recourse has not been had to that worst of controversial weapons, never employed by one capable of wielding better,—the endeavour to overwhelm an *opponent by exciting* popular prejudice against him.

"*Again, with respect to the* danger of scepticism arising from the use of mathematical reasoning, the distinction has been beautifully pointed out between the effects of *deductive* and *inductive* habits of thought*. *Deductive* habits, those acquired in drawing consequences from ascertained laws, have in themselves no tendency to direct the mind towards the contemplation of a Supreme intelligence. Whereas *inductive* habits, those which are matured by ascending from effects towards their causes, by discovering laws previously unknown, have ever been found in union with a disposition to view the great phenomena of the universe, in connexion with one Supreme intelligence and will.

"The seat of unbelief is usually, indeed, not the understanding, but the heart. It is unhappily true, that the highest attainments in all branches of philosophy and literature have not been incompatible with vague or false views of religion. But surely it is not necessary seriously to confute an argument, which would discourage the study of anatomy, because some distinguished anatomists have been suspected of materialism; which would proscribe natural history, because some of the most attentive observers have weakly regarded all the phenomena of life as the results of unconscious organization; and banish mathematics, because some of those who have cultivated them with success have reasoned ill, or decided falsely upon questions of the highest importance.

"All the sciences may be made the hand-maids of religion. And of these the severe science of mathematics, as applied to investigate the phenomena of the natural world, has ever occupied an honourable and prominent place."—(*Chevallier*, pp. 34, 35.)

Whatever may have been the reviewer's intention, it is obvious to all impartial persons, that his zeal has hurried him into a violation of sound reasoning; the tendency of one pursuit cannot be to generate two opposite defects in minds *similarly* constituted: if both these defects be generated in minds pursuing the same study, it is to the result of the constitution of the minds engaged on that pursuit, and not to the pursuit itself, that the evil is to be attributed. From what we have already urged it may be conceded, that if a mind, not naturally endowed with a sound faculty of judgment, be by circumstances trained to mathematical studies, the result may be credulity. The fact is, *credulity* and *scepticism* ought not to have been brought into opposition; they are not results of different degrees of one intellectual faculty,—imagination is always, more or less, at the bottom of the former. A person is not credulous because he is incapable of weighing moral evidence, and deciding from the balance; but he is credulous because his fancy interferes and prevents his judgment alone from acting. Mr. Dugald Stewart's assertion, that "in those who have confined their studies to mathematics alone, there has often been observed a proneness to that species of religious enthusiasm, in which *imagination* is the predominant element, and which, like contagion, is propagated in a crowd," should have been considered better, before it was cited in favour of the tendency of *mathematics* to generate credulity. The temporary prevalence of Simeonism at Cambridge, alluded to by that gentleman, is not a sufficiently decided case in point to have much weight; it was not, we believe, any new creed in religion and morals, hastily embraced, and propagated with that fanaticism and intemperance that characterize those moral epidemics, which seem to indicate especial weakness of judgment among the converts. Mr. Simeon only advocated a more strict observance of Gospel-ordinances and certain modifications of church government, concerning which men of equally sound minds and liberal education may differ in opinion: if most of those who embraced his doctrines were mathematicians, that was the necessary consequence of the majority of students at that university being such. It should have been stated what was the proportion between those who did, and those who did not, become converts to them; and again, of these two numbers, what proportion of each was to be allotted to those who studied mathematics, and to those who pursued classical literature. Unless this statement were made, the assertion is but a vague generality, of no value in such an argument.

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* Whewell's *Bridgewater Treatise*, book iii., c. 5, 6.

We think the tenour of the following passage from Mr. Stewart's "Dissertation on the progress of Philosophy," (Supplement to *Edinburgh Encyclopædia*,) is not exactly consistent with the conclusion intended to be drawn by the reviewer from those he has adduced.

> "Nothing is more interesting and instructive than to remark the astonishing combinations in the same mind, of the highest intellectual endowments with the most deplorable aberrations of the understanding, and even in numberless instances, with the most childish superstitions of the multitude. . . . . . . Nor was the study of the severer sciences on all occasions an effectual remedy against such illusions of the imagination."

As the examples of these deplorable aberrations are taken from divines, jurists, and metaphysicians*, the inference is, that Mr. Stewart, by "the severer," alluded to physical and mathematical sciences, and that he considered them, generally, as an effectual remedy for keeping down the illusions of imagination by strengthening the faculty of judgment.

Though we do not allow much weight to the opinion of individuals, as to the effects on the intellect of any study in which they either do or do not excel; we admit the full force of the evidence deduced, both from the moral conduct and intellectual cultivation of those celebrated for any particular line of study, as to the effect of that study on the mind. We bow not to the authority of Germans deciding on the inferiority of mathematical to classical, or metaphysical studies, for forming the moral constitution, because the nation is eminently unmathematical; and yet that which could give birth to, and adopt, the speculative systems of Oken, Schelling, Goldbeck, Fries, Nees Von Esenbeck, and others,—the land of mesmerism and phrenology,—the country notorious beyond every other for the universal diffusion of scepticism, for which they arrogate the term *rationalism*,—cannot, we suppose, be cited by the anti-mathematical party as having attained that healthy state of intellectual cultivation, which the line of studies pursued ought to have engendered, according to its advocates. The fact is, imagination is the national characteristic faculty of the Germans, and it will be admitted that, when not duly restrained, this faculty is the source of every moral obliquity; if Germany has produced poets, artists, musicians, metaphysicians, antiquarians, and philologists of a higher rank than other contemporary nations in recent times, it has also given birth to more credulity, fanaticism, folly, and crime.

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* The reviewer observes, triumphantly, that there are four super-eminent metaphysicians, Descartes, Leibnitz, Malebranche, and Locke; that the last was the only one who was not a mathematician, and yet how superior he was to the other three. *Malebranche was so little of a mathematician, that his acquirements in that science are unknown to all* the world but the reviewer. Leibnitz did not become a mathematician till he was twenty-three years of age, and his mind, it may be supposed, had nearly attained its full vigour. Descartes was a *genius*, like Pascal, and though inferior to Locke, yet he was only so in having been born considerably before him.

The French, as a nation, cannot be adduced in evidence on the other side, because there was till lately no general intellectual cultivation in that country: but the most inveterate enemy of abstract science as a study, must admit that, as individuals, the Sçavans of Paris are pre-eminent for their high moral and intellectual excellence. D'Alembert's virtues are universally allowed; and his contemporaries and successors in cultivating mathematical studies are, almost without exception, exempt from any stain on their character. M. Arago, in the present day, is not more distinguished for his profound mathematical and physical knowledge, than for the political influence which his firmness and rectitude have obtained for him in a country torn by factions. The late Carnot offered to defend an important fortress at a critical juncture, and Napoleon would have accepted his services. Condorcet, whose literary decisions have been pronounced by a competent authority as entitled to the highest deference, was a distinguished mathematician at sixteen. The striking and nearly single exception presented by the two Bernouillis*, to what we consider the general rule, is fully admitted; but it has such an abundance of parallels among philologists, jurists, metaphysicians, and divines, that we think no one can assert equanimity of temper, and exemption from envy, malice, and all uncharitableness, to be the usual result of *philosophical* studies, as they are termed.

We will not, for obvious reasons, cite examples of moral excellence from among our living countrymen distinguished for their scientific attainments; yet we cannot refrain from alluding to one, because the attainment of profound knowledge in abstract science being rare in an individual of her sex, such an example naturally presents itself in a discussion on the beneficial effects on the moral character of such studies. If, then, to a knowledge rarely equalled by men, be united every quality that can adorn a woman, that knowledge being enhanced by the possession of extensive acquirements in other studies—we may at least be allowed to quote her opinions, her pursuits, and her example on our side, in contrast to those of the culpable De Staël.

Let any one recall to his mind all the quarrels which have distracted the world of literature, from the earliest ages to the present moment, let him reflect on the bitter, relentless, bloody hostilities, excited by purely speculative matters of opinion, such as those between the Nominalists and Realists, between the Roman and Canon jurists, the Jansenists and Molinists, with numberless others to which we will not allude; and then let him decide whether the cultivators of abstract and physical science have not been, at all periods, distinguished for their unanimity and moderation. But we quit this argument, forced on us by the imputations we have been considering, to return to one more general and more likely to conciliate.

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* We say nearly single, for the unfortunate differences between Newton and Flamsteed, which have lately been brought to light, are venial, compared to the unnatural hostility of the Swiss brothers; and the exact proportion of blame to be imputed to Flamsteed or his great opponent, is still a matter of discussion.

If the intellectual faculties are generally referrible to those of judgment and imagination, and the predominance of one class over the other primarily determines each individual mind to those pursuits which more particularly demand the exercise of that one: then the question before us is at once freed from all considerations of imagination, since its influence, when felt, is too powerful to admit of more than control, and is little susceptible of cultivation. When, therefore, the individual is not endowed with that rare genius which renders him a benefactor to mankind, by the happiness his creations as a poet, a painter, or a musician, excite in his own and future ages; the aim of the liberal education he gives himself, that is of his pursuits after he has passed through the discipline of school, should be the improvement of his judgment, so as to preserve his imagination subordinate without suppressing it: by so doing, he will most probably secure the maximum of happiness; the undue influence of ordinary imaginations, when unchecked by the judgment, being the source of all the evils which disturb the peace of the individual, as surely as its dominion, when it is of an exalted nature, is productive of the greatest good to society.

By not suffering any extrinsic cause to induce him to pursue one line of studies in preference to another, but by adopting that for which he feels an especial inclination, he will most certainly attain the end in view, and be secured from the exclusive effects of one study by the invariable tendency of each, if pursued with steadiness and energy, to lead to and connect itself with others. The endeavour to exalt the importance of one line of studies at the expense of another, is particularly to be deprecated, not only as causing an undue influence in determining the choice of pursuits, which, in our opinion his own feelings—the suggestions of the individual's mind,—ought alone to decide; but as fostering those bickerings and animosities so detrimental to the general intellectual advancement. Instead of exciting the philologist, the historian, the metaphysician, the naturalist, the politician, and the mathematician, to regard with jealousy, or to treat with contempt, the pursuits of the others, true philosophy should point out to each, that they are all equal in dignity if they contribute to the real happiness of the individual, by alluring him from the debasing tendency of sensual pleasures; and the contest among the different cultivators of knowledge should be to prove the beneficial effects of each species on the mind, by sedulously cherishing courtesy and charity towards their fellow-labourers in the vineyard, and not to flatter their own indolence or presumption by striving to depreciate the utility of exertions of which they are incapable, or to which they are averse.

A POPULAR COURSE OF ASTRONOMY.

No. IV.

In the last chapter was pointed out to the reader a method readily applicable to the determination of the difference of latitude of any two places on the earth's surface. It was simply to observe the number of degrees through which any of the fixed stars had been made apparently to ascend or descend on the vault of the heavens by the change of the position of the observer.

But every star has another *apparent* motion besides that which arises out of an alteration in the place from which it is observed. There is an apparent motion from east to west, which is *common* to the whole host of the stars, and by which each star is made to describe about 15 degrees of the circle, which is its apparent path, every hour. Hence, whilst an observer is changing his place and his latitude, the star which he has observed will itself have had an apparent motion towards the east, arising from a cause quite independent of his changed place, and out of the whole apparent motion of the star he will find a difficulty in ascertaining how much is, and how much is not, due exclusively to this change of place.

Fortunately, however, the star will return after 24 sidereal hours, or $23^{\text{h}}\ 56'\ 44{\cdot}09''$ mean solar time, exactly to the same place in which it was at the period of his first observation; and if he repeat his observation at that moment, the result will be precisely the same as though the stars had not moved at all in the interval.

Thus, then, if he makes his second observation precisely $23^{\text{h}}\ 56'\ 44{\cdot}09''$ after the first, he knows that any change in the apparent altitude of the star must arise from his motion, and not from any apparent motion proper to it. And this remark applies to all that has been said before of that apparent motion of the stars which is produced by a change in the position of the observer.

This subject will, however, be better understood, when the reader's attention shall have been called more specifically to the diurnal motion of the earth.

It has been shown that the earth is a huge isolated mass, having no contact with any other, but self-supported in space.

Now it will at once occur to the reader, that a mass, placed under these circumstances, whose surface had no other contiguous surface to rest against, no fixed pedestal or suspending-chain to keep it in its place, would necessarily move, if any external force were applied to it. And those who have studied the theory of mechanics know further, that any motion thus communicated to it would, since there is no friction or other opposing resistance to destroy it, continue for ever. And that this is true as to the *fact*, however great or however small may be the amount of the disturbing force, varying only in this respect as to the *degree* of the motion. It will occur to them, therefore, as quite possible that this ball should be, and should *have* been from all eternity, in motion, provided there be, or ever have been in existence, an external power ble of moving it. Nay, their speculations on the probability of the case may be carried yet further. It is a principle of mechanics, that if motion be communicated, by impact or otherwise, to a mass in any other direction than *through* its centre of gravity, this mass, when left to itself, will have two motions, one a motion of translation, in which all its parts, including its centre of gravity partake in common—the other, a motion which will ultimately be a motion of rotation about a certain axis through its centre of gravity, in which only those parts of the body which are *without* this axis will partake. And it is a remarkable fact that these two motions of translation and rotation will be quite independent of one another, so that the motion of translation will be precisely the same as though the mass had been struck through its centre of gravity, and there had been no rotation, and the motion of rotation the same as though there had been no translation, the centre of gravity of the mass having been held at rest. Thus, were the mass a sphere, and had it been struck otherwise than through its centre, it would necessarily spin round one of its diameters, and at the same time move forward in a straight line, with a motion of translation. Also this spinning motion would be the same as if the axis about which it takes place had been kept at rest like that of a globe, and the motion of translation the same as though the ball had been struck through its centre, and had not therefore spun at all on its axis.

And all this is true, however slight the impulse which might be given to it.

To put this fact in a more striking light, let us suppose the force of gravity on the earth's surface for an instant to be *destroyed*, and let the reader be imagined to have constructed a sphere of clay, and having done so, to hold it up in his hand, and then to unloose his grasp from it. It would immediately begin to spin upon one of its diameters, and to move onward through space with an uniform motion, which would never of its own accord alter its direction, or cease. There being no force of gravity to draw it downwards, had no force whatever been communicated to it when it was set free, it would have *rested;* but it will have been found impossible to set it free from the hand without communicating some motion to it, and it is an infinity of chances, that the direction of that motion shall not have been precisely through its centre; in which case there will, of necessity, have resulted a motion of translation, and one of rotation.

It is scarcely necessary to apply this illustration to the case now under our discussion: that the Hand by which the materials of our globe were brought together could have been withdrawn, and yet that mass left quiescent in space no one ventures to deny; but that it should move is the simpler case, and that the same Hand, when it had spread upon the face of the earth its glorious covering of green herbage, of flowers, and of forest-trees, and sent forth the cattle on a thousand hills, should then have imparted to it that impulse in space, whence should result the alternations of day and night for the repose of every living animal, and the periodical changes of heat and cold, whereby every variety of vegetable life should be made to bring forth its fruit in due season, is by far *the more probable of the* two suppositions.

That this earth, then, which we *know* to exist, isolated in space, should be in motion, that it should revolve continually and uniformly round one of its axes, and at the same time with a motion of translation *forward* in space, will not, therefore, seem improbable. In fact it is seen on the whole to be more probable than that it should be at rest.

Let us now consider the matter in another light. In a former chapter, an observer was supposed to set out from the north, and travel southward round the earth; and it was shown that the horizon of such an observer must (to explain the phenomena) be supposed continually to roll with him, causing by its angular *approach* to some of the fixed stars as it thus rolled along, and its recession from others, an apparent approach of the stars northwards to the horizon, and their ultimate immersion beneath it, and the converse of all this southward. Now, instead of his moving southward from the north, let us suppose him to move eastward from the west. His horizon now, as before, rolling along with him, those stars which are behind him will continually appear to descend upon the vault of the heaven behind him, and those before him to ascend; thus they will appear to rise to the eastward, to revolve over his head, and to set in the west. Let him now be supposed to move thus with such rapidity as to describe in twenty-four hours the whole circumference of the earth, and to continue this gyration uniformly and unconsciously for ever. As his horizon is thus brought continually into different positions, with reference to the stars, and as he does not suspect the fact of the motion of his horizon, he will necessarily suppose the stars themselves to take up different positions with regard to the horizon, to ascend from beneath it, pass over the space above it, descend again beneath it, and every twenty-four hours, to make a complete revolution about him. Now, instead of the observer thus careering continually round the earth, let us suppose him to remain at rest, and the earth itself to move, carrying him round with it. The appearances of the heavens will manifestly be to him exactly the same as before.

The only difference of the cases is this; instead of the observer having in every position a new horizon, occupying a different situation with regard to the region of the fixed stars, he will have everywhere the same horizon, which will be made to occupy in succession precisely the same positions as his *different* horizons did on the former supposition; and being here, as before, unconscious of the motion of his horizon, he will attribute the apparent ascent of the stars to the eastward, and their descent westward, to a proper motion of the stars themselves, and not to its true cause the alteration of the position of his own horizon with respect to them.

Thus, then, if the earth carried us round perpetually in space as it spun upon its axis, looking at the stars we should observe precisely the same phenomena as those which the appearances of the heavens daily present to us. The heavens would appear to turn round us. We have then to choose between two hypotheses, which equally well account for the observed facts of the apparent daily rotation of the vault of the heavens; these hypotheses are, that the host of heaven do daily revolve with a common motion round us, or that the earth revolves daily and uniformly round one of its diameters.

We have shown this last hypothesis to be in the highest degree probable from the fact, that the earth is a mass, separated and isolated from all others, and, as it were, self-supported in space. Certain, therefore, to retain any motion communicated to it, and if that motion were communicated otherwise than through its centre, certain to revolve for ever upon one of its axes, as well as to move forwards.

Now let us consider the probability of the other hypothesis, *viz.* that the heavens and all their host do really revolve round us every twenty-four hours as they appear to do. It has been shown in a preceding chapter, that the region of the fixed stars is distant from us by a space not less than one hundred thousand times the earth's diameter—in reality it is far more remote than so many times the diameter of the earth's orbit. Being thus distant, the magnitudes of the fixed stars must be enormous, or we should not be able to see them.

The hypothesis of a daily revolution of the heavens amounts then to this, that millions of immense bodies, stars innumerable, revolve each in its particular orbit, and each with a velocity greater than that of light round one of the axes of this earth of ours, which is but an atom in comparison with the least of them. There is a limit somewhere placed, beyond which that which is improbable identifies itself with that which is impossible, and this hypothesis seems to pass it.

The improbability may, however, yet be rendered stronger. The stars, called fixed, because they preserve always the same relative positions, are not the only stars seen in the heavens, there are other bodies, whose apparent positions in reference to one another, and to the fixed stars, are pepetually changing, "Palantia sidera cœlo." Besides, then, their daily revolution with the rest of the heavens, these must, if our hypothesis be true, have a continual motion among the other stars, and this of the most perplexing and extraordinary kind. The sun, for instance, must be supposed, besides his daily motion, to move in the same direction completely through that girdle of stars called the zodiac, once a year; and the moon once a month. The planets Mercury and Venus must be supposed always to accompany the sun in his motion, but sometimes to lag behind him, and at others, to press on before him, altering perpetually in brightness with each variety of motion. The planets Mars, Jupiter, and Saturn, must be supposed to have paths, subject to so complicated a law of change, as to appear to have their motions governed by a kind of caprice, and to render it difficult to say whether

'Sponte sua jussæne vagentur et errent.'

Sometimes we must suppose them to travel forwards on the vault of the heavens, then by an indirect and tortuous course to retrograde, at one time in opposition, at another, in conjunction with the sun, thus presenting the image of a wandering, unsettled, reeling, and lawless course through the sky; and all this motion, which passes through its changes slowly, and by periods of months, combined with the steady and regular daily motion, common to the whole region of the stars. The complexity of this hypothesis renders it next to impossible that it should proceed from the *same Hand, of whose skilful* operation we find such abundant *evidence in the things* that surround us.

Now let us place the hypotheses together; on the one hand we have to suppose that these millions of stars, situated at immeasurable distances from our earth, and immeasurably greater than it, nevertheless whirl round it with inconceivable rapidity every twenty-four hours, and that besides this motion, certain of them wander perpetually through space in tortuous eccentric paths, subjected to some unknown and most complicated law of deviation.

On the other hand, take the hypothesis, that the earth revolves upon its axis perpetually and uniformly, and at the same time moves forward in space, an hypothesis rendered in the highest degree probable by the fact, otherwise ascertained, of its entire isolation in space.

The improbability of the first hypothesis, infinite as it is in itself, is infinitely increased by the probability of the second.

But however conclusive may be this balance of probabilities, the question admits of a still more rigid determination.

In the first chapter were stated the circumstances which absolutely prove the stars to be material bodies like our earth, subject to the same laws of attraction and motion as what we see around us; and the same is ascertained with equal certainty in respect to the sun and planets of our system. Now this being the case, it is *impossible*, from the nature of these laws of attraction and motion, that this sun, these planets, and these immense and distant stars, should turn continually round our little earth.

If two bodies, subject to the known laws of attraction and motion, revolve freely in space, we know that their revolution must take place, not about the actual centre of gravity of either body, but about their common centre of gravity. Now the common centre of gravity of two bodies is nearer to the *greater* of the two; so that the point about which the two revolve is always nearer to the greater body; and if the one body be infinitely greater than the other, it is infinitely nearer to it. And thus the effect is precisely the same as though the less body revolved about a point coincident with the centre of gravity of the greater.

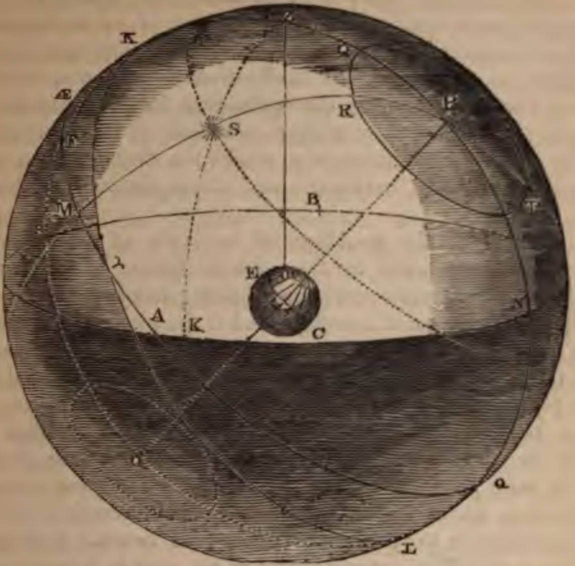
But the sun is infinitely greater than the earth. The sun could not, therefore, if the earth and sun only were in existence, revolve round the earth, but the earth must revolve round a point infinitely near to the centre of the sun. And this result will scarcely be affected by the introduction of the other bodies of our system into the discussion;—the whole revolve about their common centre of gravity, which by reason of the great magnitude of the sun, when compared with any of them, is a point which may be considered as fixed, and which may be considered as exceedingly near its centre.

It is impossible, then, that the sun should revolve round the earth every twenty-four hours. And we must take the other hypothesis.

The earth turns upon one of its diameters called its axis every twenty-four hours, thereby causing that vast, hollow sphere, whose centre it may be imagined to occupy, to appear continually to revolve round it in that period.

Let us imagine the axis of the earth to be produced both ways, so as to meet the surface of this great sphere of the heavens in the points

P and Q. It will thus mark out the two poles of the heavens, about which the stars appear to have their diurnal paths, of which one is represented by the circle, R T, in the figure.



Let the plane of the equator of the earth, E C, be produced, to intersect the sphere of the heavens. The great circle, Æ Q, in which it will thus intersect it, will be the equinoctial. Any plane drawn through the axis, P Q, of the heavens will intersect the celestial sphere in a circle called a declination-circle, of which circles P S M, shown in the figure, is one. Declination-circles are thus great circles which pass round the heavens from one pole to the other. Every position of the heavens is supposed to have one of these declination-circles passing through it. The use of them is to fix the position of any star on the vault of the heavens, in the same manner as the position of a place is fixed on the surface of the earth by its longitude and latitude.

If we know the particular declination-circle which passes through any star, and also the situation of the star on that circle, we have an accurate conception of the position of the star on the vault of the heavens. We can convey that conception to others, and by reference to a celestial globe, or to a chart of the heavens, we can tell what this particular star is, and what is its position in reference to other stars.

Each declination-circle passes through the poles of the heavens, and, of course, intersects the equinoctial, which lies midway between these poles at right angles. There is a particular point on the equinoctial, called the point Aries, marked in the engraving by the symbol ♈, the position of which in the heavens will be explained hereafter The distance of the point where the declination-circle of any star cuts the equinoctial, from this point, Aries, being measured eastward along the equinoctial, is called the right ascension of that star; and the distance of the star from the equinoctial, measured on its declination-circle, is called the declination of the star. Thus, knowing the right ascension and declination of a star, we know its exact position on the great sphere of the heavens, and can refer to it on a celestial sphere or chart; for from the right ascension we know the position of its declination-circle, and from the declination, its situation on that particular declination-circle. Thus, in the figure, the declination-circle, P S M, which passes through the star, S, intersects the equinoctial in the point M, the distance of this point from ♈, measured eastwards on the equinoctial; Æ Q is therefore the right ascension of S, whilst the distance, S M, measured on the declination-circle, between S and the equinoctial, is the declination of S.

If the plane of the meridian of longitude of any place on the earth's surface be continued to the celestial sphere, it traces out there what is called the celestial meridian of that particular place. Thus, if O be any place on the earth's surface, and if the plane of the meridian of longitude passing through O be produced to intersect the sphere of the heavens, the circle in which it will intersect it is the celestial meridian of O; it is represented in the figure by the circle N P Z K F Q.

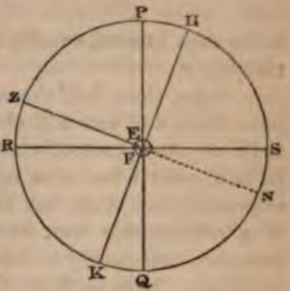
Since the earth is continually revolving in the position which it apparently occupies in the centre of the celestial sphere, the celestial meridian of each particular place is continually revolving over the face of the heavens, about its axis, coinciding in succession with all the declination-circles in the course of twenty-four hours. This is the real state of the case. The *apparent* state of the case is, however, precisely the *opposite* of this. The place of the observer appears to be fixed, and therefore, his celestial meridian to be *fixed;* whilst the stars, and with them their declination-circles, appear to revolve every twenty-four hours, each declination-circle coinciding in its turn with his meridian.

When the declination-circle of any star thus coincides with the celestial meridian of any place, the star is said to be *on* the meridian of that place, and its altitude at that moment above the horizon, is called its meridian altitude.

The plane of the meridian passing through the axis of the earth passes through its centre, and is perpendicular to its surface. A line perpendicular to the earth's surface at any point, is, therefore, in the plane of the meridian at that point, and such a line being produced to the heavens, will intersect it in a point of the meridian of the place.

Thus the vertical, O Z, at any place, O, on the earth's surface, being produced to the heavens, intersects them in the celestial meridian of the place. The point, Z, where the vertical intersects the sphere of the heavens, when produced upwards, is called the Zenith; when produced downwards, the Nadir. The Zenith is that point of the heavens which an observer sees immediately above his head; the Nadir, that point which he would see if nothing intervened immediately beneath his feet.

The celestial meridian of any place has been shown to pass through its zenith. Also, by the definition of it, it appears that it passes through the poles of the heavens. The celestial meridian of any place is thus a great circle drawn through its zenith and the poles of the heavens. The points where this circle meets the horizon are called its north and south points, and the points of the horizon half-way between these, its east and west points. Thus, if N B S A be the horizon of an observer at O, the points N and S, where the celestial meridian of that place intersect it, are its north and south points, and A and B, half way between these, its east and west points. If a great circle, Z S K, be imagined to be drawn from the zenith, Z, to the horizon, through any star, S, it is called the azimuth circle of that star, K S is its altitude, Z S its zenith distance, and N K its azimuth.

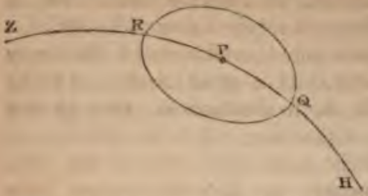


Let P represent the pole of the heavens; Z, the zenith of an observer on the earth's surface at E; P Z Q S H, a great circle of the heavens passing through these points: this circle is, therefore, the meridian of the observer at E. Let H K be the horizon of the observer at E, at right angles to Z E; also let R S be the equinoctial at right angles to the axis, P Q, of the heavens. The earth, E, may be considered as a mere point, in comparison with the sphere whose centre it occupies. Now the celestial meridian, P Z K S, being in the same plane, and concentric with the *terrestrial* meridian of the observer, the arc, Z R, between the equinoctial and the zenith, contains as many degrees as does the arc, E F, of the *terrestrial* meridian between the equator and the place of observation. In fact, these arcs measure the same angle at the earth's centre. But the arc, E F, of the meridian intercepted between the equator and the observer's place, is his latitude; the arc, Z R, between the equinoctial and the zenith, is, therefore, equal to the latitude. And if we could but see exactly where the equinoctial was in the sky, if it were marked, for instance, upon it as it is upon our globes, by a band stretching across the heavens, we could at once determine the latitude of any place by measuring the distance upon the meridian between this band and the zenith of the place.

But although we cannot, without much difficulty, fix the position of the equinoctial in the heavens, the pole is much more readily found; and this will answer the same purpose, for the arc, Z R, is equal to P H; therefore the arc, P H, which is the distance of the pole from the horizon, or the elevation of the pole, as it is termed, is equal to the latitude of the place of observation. Here, then, is a very simple method of determining the latitude. We have only to observe the altitude of the pole of the heavens above the horizon.

But there is still another difficulty; for the pole of the heavens cannot at once and accurately be found. The polar star is usually said to be in the pole of the heavens, whereas it is, in reality, distant from it by about one degree and a-half.

How, then, shall we find the exact height of the pole, not being able to distinguish its place in the heavens. *Thus*, let us fix upon one of those stars which are not so remote from the pole as to be made by their revolution round it, to sink beneath the horizon, and are, therefore, called circumpolar stars.



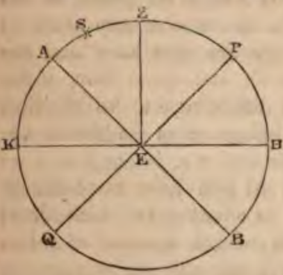
Let R Q represent the diurnal path of one of these about the pole, P; also, let Z R H be the celestial meridian of the observer.

Let the altitude of the star be observed when it is on the meridian at R, at what is called it *superior* passage over it, and also when at Q, at the time of its *inferior* passage; the altitudes H R and H Q being thus known, exactly half their sum will be H P, the exact height of the pole P.

Take, then, half the sum of the two meridian altitudes of a circumpolar star, and you will obtain the altitude of the pole that is the latitude of your place of observation. It is clear that the star is at its highest point when at R, and at its lowest at Q. The rule, then, may be expressed thus: take half the difference of the greatest and least altitudes of a circumpolar star, and the result will be the latitude. Thus it becomes unnecessary to know exactly what is the position of the celestial meridian. This is probably the most accurate method of finding the latitude.

The practical objections to this method of determining the latitude, are these,—it requires an interval of the half of a sidereal day between the two observations required; and it requires that the observer should remain in the same place during that interval. Now the latitude is sometimes required to be known at once, and, as in the case of a ship at sea, the same place cannot be retained during the interval in question. Again, these are observations which can only be made at night.

The following method will obviate all these difficulties.



Let S represent the position of any of the heavenly bodies, a star for instance, or a planet, or the sun, or moon, when on the celestial meridian of the observer.

This celestial meridian, then, coincides with the declination-circle of the star, and the distance, A S, of the equinoctial from the star measured on the meridian, is the declination of the star. Now suppose the declination to be known,—and the declinations of all the principal stars are known, and have been inserted in tables; also the declination of the sun, which alters daily, but nevertheless admits of being calculated for every day of the year; and is so calculated and registered in the *Nautical Almanack*. Hence, therefore, the distance, A S, of the sun, or star, from the equinoctial, is *known* for every day in the year. Now, let the meridional distance of the sun from the ZENITH be *observed*, if the latitude be required to be found in the day-time, or the meridian distance of a known star from the zenith if the latitude be required at night. Thus, the distance S Z will be known, and A S, the declination, is known by the tables. The sum of these, A Z, is the latitude. If it be more convenient to measure the distance of S from the horizon than the zenith, as is commonly the case, then from this meridian altitude of the sun, as it is called, its zenith distance, S Z, is at once found, by subtracting it from Z K, which we know to be 90°.

It need scarcely be suggested that when the altitude of the sun is thus measured, it must be the altitude of the centre of his disc, it being the centre of the sun's disc of which the declination is given in the tables.

Some of the most simple, and at the same time some of the best, of the numerous methods for determining the latitude having been explained, let us now consider how that other element requisite for ascertaining the place of the observer on the earth's surface—the longitude may be found.

The motion of the earth upon its axis is UNIFORM; hence, therefore, it follows, that the celestial meridian of every place on the earth's surface is swept over the face of the heavens with an uniform motion. Since the earth revolves in $23^h\ 56'\ 44{\cdot}09''$, or nearly twenty-four hours, *completely* upon its axis; the meridian of each place travels the heavens at the rate of about 15° an hour.—The reader must here bear in mind that the earth is supposed to be revolving in the centre of a fixed hollow sphere, and that the celestial meridians of places on the earth are great circles of this great outer sphere, revolving round the axis of the heavens, with those places on the earth's surface to which they severally belong, and thus coinciding in succession with the declination-circles which are fixed on the *concavity* of the sphere.—Now let us suppose one of the terrestrial meridians of longitude to pass through each degree of the equator: for each of these terrestrial meridians there will be a corresponding celestial meridian, and each such celestial meridian sweeping the heavens at the rate of 15° per hour, fifteen of them will pass over the same point of the heavens every hour, and generally the celestial meridians will pass over the same point in the heavens, the same star for instance, (the same sun, or the same planet,) at the rate of 15° per hour. Hence, therefore, if the times at which the celestial meridians of two places coincide with the same star, differ by one hour, we know that the terrestrial meridians to which they belong are 15° apart, or that there is 15° difference of longitude between the two places.

Thus, then, if there were two observers at two different places on the earth's surface, and they had clocks set exactly to the same time; then, if each observed the time by his clock of his celestial meridian passing over a certain star (that is of the star apparently coming on his meridian), then the difference of these times, or the interval shown by the clocks between them, would show the number of degrees of longitude which intervened, allowing at the rate of 15° per hour, or a degree for every four minutes of difference of time. If one observer, for instance, found that the star came on his meridian at eleven at night by his clock, and the other saw it at twelve minutes past eleven by his clock, then they would know that there were 3° difference of longitude between the places of observation.

The actual longitude of a place is the number of degrees of longitude intervening between it and the meridian of the observatory of

Greenwich. Hence, if one of the observers of whom we have spoken were at Greenwich at the time of his observation, the difference of longitude deduced from the two observations would be the actual longitude of the place of the second observation. The inconvenience of this method is manifestly this;—that the two observers would be obliged, however remote their stations, to come together and compare their results, before anything would be known.

Now instead of two observers, let us suppose one of them to have observed at Greenwich the time when a particular star passes the meridian by a clock set to sidereal time,—that is so set that the hand revolves precisely twice round the dial-plate in the time which intervenes between the transit of a star over the meridian and its return to it, which time is always the same,—he knows, then, that the star will always return to the meridian of Greenwich precisely at the hour thus shown by his clock. Let us suppose it to be ten o'clock. Let him now set out to some other place westward, taking his clock with him. Let him thus travel for six months, and after that time wish to know into what longitude he has got. He has only to observe at what hour by his clock the star which he observed at Greenwich now passes over his meridian. Suppose it one o'clock at night. If his clock has gone right during the intervening six months, he knows that the star passed that night over the meridian of Greenwich at ten o'clock, that is three hours before; allowing, therefore, 15 degrees of longitude an hour, it follows that he is 45 degrees west of Greenwich.

Now this is precisely the way in which the longitude is commonly found at sea, except that it is not pendulum-clocks which are used there, these being of course subject to injury and derangement, from the motion of the ship, but CHRONOMETERS, which are now constructed so as to go with wonderful accuracy, under every change of circumstances and temperature. Instead of one chronometer, several are usually taken in the same ship. So that if any one by chance go materially wrong, the error will be detected by its disagreement with the others. Chronometers are commonly set, not for sidereal time, but for mean solar or common time; but then the difference between this and sidereal time being known to be 3′ 56·5″ daily, it is very easy to ascertain what the sidereal time is from the common time.

By observations on the stars, the longitude can only be ascertained at night. Now it is frequently very desirable to find it by day.

How shall this be done?

The principle on which the determination of the longitude by means of a star is founded, is this, that we know the time when the meridian of Greenwich passed over the star, and observe when our own passes over it. Now suppose we know when the meridian of Greenwich passed over the sun, and observe when our own meridian passes over the sun. It is evident that we shall know, making the allowance of 15° per hour*, precisely as before, what is the difference of longitude.

But how can we tell at any remote place *when* the meridian of Greenwich on that day passed over the sun? If the meridian came back to the sun every day after the same interval, that is, if the solar days were all of the same length, there would be no difficulty whatever in this, we should only have to set our chronometer to solar time—put the hand at twelve o'clock, when the sun was on the meridian at Greenwich, then travelling anywhere else, we should know that if the chronometer kept correct time, when the hand was again at twelve, the sun would be on the meridian at Greenwich. And if we observed what time was shown by it when the sun was upon the meridian at the place of our observation, the difference between this and twelve o'clock, allowing one degree for every four minutes of time, would give us at once the longitude. But the sun does not return to the meridian on *any* two successive days, after precisely the same interval; so that the chronometer, whose hand returns to twelve *precisely* after the same interval, cannot show for any two successive days the precise time of the sun's passage, or rather of the passage of the meridian across it, and our method fails. How then shall we get over the difficulty? Thus;—the difference between the mean solar time of noon, or twelve o'clock, and the time when the sun actually comes to the meridian, may be previously calculated for every day of the year, and it is so calculated and registered in the *Nautical Almanack*. Hence, therefore, if we have a chronometer set by Greenwich, which keeps true mean solar time, we can tell by reference to the almanack how much before or past twelve o'clock it is by that chronometer when the meridian of Greenwich passes over the sun; and observing when the meridian of the place where we are passes over the sun, we have the difference of the times of the two transits, and allowing 15° 2′ 27·847″ for every hour, or 15′ 2·041″ for every minute of this time, we obtain at once the longitude of the place. The reason why we now allow somewhat more than 15° for the motion of the meridian per hour is, that now our chronometer shows mean solar time, whereas before we supposed it to show sidereal time. The nature of this difference will shortly be explained. But first it will be well to point out certain methods by which the longitude may be determined, which do not depend for their accuracy, as these do, upon the rates of chronometers. It is very important to have such means, because, after a long period of time has elapsed, it is scarcely possible but that the greater portion, if not all of the chronometers may have failed, and the true time have been lost. Or it may be necessary to determine the longitude of a distant place with greater precision than any chronometer can possibly give it.

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* Making also a small allowance for the apparent motion of the sun.

All that we want to determine is, on any day, when we are at a distant place, the precise moment when the meridian of Greenwich passed, that day, over the sun. Now suppose that precisely at that moment they threw up a rocket at Greenwich which we could see at the place of our observation: this would completely answer our purpose, for looking at the clock when we saw this rocket, and observing the precise moment by the same clock when the meridian, where we are, passes the sun, the difference of these times, properly reduced, will give us the longitude. This rocket need not, however, be thrown up precisely at the moment when the meridian of Greenwich passes the sun. Provided it be agreed on or known before precisely how many hours, minutes, &c., it is before or after the meridian transit that the rocket will be thrown up, we are manifestly just as well able to tell what was the precise moment of transit as though the rocket went up when it actually took place.

Well now, instead of the throwing up a rocket, let us suppose that the astronomer at Greenwich could put a screen over the moon, or over one of the satellites of Jupiter, precisely at a number of hours before or after the transit of the sun at Greenwich, which number of hours was known and agreed upon beforehand. This phenomenon, wherever it was visible, would answer the purpose of the rocket, and enable us to tell the time of the transit of the meridian of Greenwich; comparing which with our own, we should know the longitude.

Now this is what actually occurs, except that it is not the hand of the Astronomer, but that of GOD, which, at appointed seasons, brings darkness upon the face of the moon, and causes night after night one or other of the satellites of the planet Jupiter to plunge into his shadow. The precise number of hours, minutes, and even seconds, before or after the transit of the sun at Greenwich, when these phenomena occur, are calculated and registered in the *Nautical Almanack;* and any one observing them at ever so distant a place, can tell thus the time of the sun's transit at Greenwich, and observing his own, he can thus find his longitude.

The eclipses of the sun or moon occur but rarely, so that an opportunity of finding the longitude by them, although certainly the best, is very seldom presented to us. The eclipses of Jupiter's satellites occur almost nightly, and these answer every purpose of finding the longitude on land. But at sea this method fails, for it is impossible to hold a telescope of the required length sufficiently steady on ship-board to see the satellites of Jupiter.

In the failure of these methods, another of great ingenuity has been contrived—a method which, in its practical application, simplified as it is by the use of tables, presents little or no difficulty, but which, in the researches on which those tables are based, constitutes one of the greatest triumphs that the human intellect has in our times achieved.

The moon does not rest among the fixed stars, but moves along that band of the heavens which is called the zodiac; and it moves with a comparatively rapid motion, describing the complete circuit of the heavens in $27^{d}\ 7^{h}\ 43'\ 4''$, or moving at the mean rate of $13°\ 10'\ 35''$ a day, and $32'\ 56{\cdot}46''$ per hour. Thus, then, the moon is in no two successive hours at the same distance from any one of the fixed stars, nor, indeed, in any two successive minutes. Suppose, now, that the time after or before the sun's transit at Greenwich, when the moon would be at a given angular distance from a certain fixed star, were calculated, and inserted in the *Nautical Almanack*, and that an observer at a distant place, having that almanack, were to observe when the moon was at that angular distance from the star, (an observation which he could very readily make, even on ship-board, by means of an instrument called a sextant,) he would know precisely how far the moment when this observation was made, was from the time of the sun's transit at Greenwich; and having observed when the sun's transit took place with him, he would thus, as before, have the difference of longitude.

BOTANICAL RAMBLES IN THE VICINITY OF DOVOR.

No. III.

Having in my two former rambles examined the country to the east and north of the town of Dovor, I shall in the present one take the reader in quite an opposite direction; and, starting from the Priory, we will first examine the fields which lay to the right towards Buckland, then crossing over to the back of the Heights, we will search that neighbourhood, and then proceed up the valley towards Folkstone, along the high road. Having reached the head of the valley, we will descend the cliffs, and examine Lydden-spout and the under-cliff, returning to Dovor over the downs, *via* Shakspeare's Cliff. The fields to the right of the Priory are mostly arable, and will yield us common Fool's-parsley (*Æthusa cynapium*), Needle-chervil (*Scandix pecten*), Pansy-violet (*Viola tricolor*), annual Mercury (*Mercurialis annua*), and Milk-thistle (*Carduus marianus*), with its beautifully veined leaves. Crossing from these fields to the back of the Heights, we shall find on the parts of the down adjoining the cultivated ground, some good localities for plants; but we shall only have to notice greater Burnet-saxifrage (*Pimpinella magna*), and hairy St. John's-wort (*Hypericum hirsutum*); the former growing in bushy places, on the slope of the Heights, and the latter in and about the hedge-rows, surrounding the cultivated ground.

Returning now to the Folkstone road, we will proceed up the valley, and shall observe as we pass along, stinking Iris, (*Iris fœtidissima*), nettled-leaved Bell-flower (*Campanula Trachelium*), and common Fleabane (*Pulicaria dysenterica*), on banks by the road side. The ripe seed-vessel of Iris fœtidissima is a very beautiful object. It opens into three divisions, each displaying two rows of brilliant scarlet seeds, and if gathered at the proper time, the seeds will remain on the divisions of the seed-vessel for a long time, and form a pretty ornament in the winter season, when flowers are scarce. In the fields adjoining the road, particularly to the right under some coppices which hang on the brow of the hill, nearly at the top of the valley, we shall find Field Scorpion Grass (*Myosotis arvensis*), Corn-parsley (*Petroselinum segetum*), hedge Bastard Stone-parsley (*Sison Amomum*), red Campion (*Lychnis dioica*), Henbit-nettle (*Lamium amplexicaule*), lesser Calamint (*Calaminta nepeta*), knotted Figwort (*Scrophularia nodosa*), and bristly Ox-tongue (*Helminthia echioides*).

About three miles on the Folkstone road we reach the head of the valley, and arrive on very high ground, having to the left a station-house on the edge of the cliff. Here we shall find a circuitous and steep path, which will lead us to the under-cliff surrounding East-Wear Bay, (sometimes called Folkstone under-cliff), and Lydden-spout. As we descend, a beautiful scene presents itself. The under-cliff lies stretched beneath us, extending to Folkstone in one direction, and for some distance towards Dovor in the other, formed of a series of rounded chalk undulations, covered with soft verdure, intermingled here and there with underwood bushes and brambles. A smooth and beautiful beach of sand and shingle bounds it towards the sea, offering a most inviting spot to those who feel inclined to luxuriate in the pleasure of a sea-bath. The cliffs on the land side, not being very perpendicular, are covered in many places with vegetation, and offer a more pleasing outline to the eye than the precipitous cliffs usually seen on this part of the coast. The most beautiful and wooded part of the under-cliff is that towards Dovor, and to this we will pay the most attention, for we shall find more to attract us here; and if we ramble towards Folkstone we shall be straying too far from home.

To the right of the path which leads down the cliff, on some bushy slopes near the bottom of the descent, we shall find large beds of strong-scented Lettuce (*Lactuca virosa*), an useful medicinal plant, but little prepossessing in appearance or odour, the latter being very rank and unpleasant. A thick white juice exudes from any wounded part of the plant, which is very troublesome to those who handle it; it soils cloth very much, and the stain is difficult to remove, as I know from experience. Continuing to the left towards the beach, we shall come to a little stream, running out from beneath a bed of clematis and brambles, close to the sea-shore, where it loses itself among the shingle. Brook-weed or Water Pimpernel (*Samolus Valerandi*), grows near this spot, in damp places; and on the sloping banks on the Folkstone side, we shall observe wild Madder (*Rubia peregrina*), growing in abundance, and pyramidal Orchis (*Orchis pyramidalis*). A little further on in the same direction, high up, on a chalk slope, there is quite a bed of smooth Sea-heath (*Frankenia lævis*), which is well worth visiting, to observe this plant growing in luxuriance.

In my first ramble I gave localities for the wild Madder and smooth Sea-heath; but as both plants are rare, I have mentioned them here again, as they seem to grow more luxuriantly, and be more abundant than on the other side of Dovor.

On and about the sea-shore common Hound's-tongue (*Cynoglossum officinale*), is to be met with in many places, and the common wild Parsnep is most abundant in this under-cliff, growing in large beds, to the no small satisfaction of myriads of insects, which enjoy the flowers when open.

The reader will find by referring to G. E. Smith's *Catalogue of the Plants of South Kent*, that several of the Orchis tribe, and other rare plants, are to be found in East-Wear Bay, and at Lydden-spout; but they were all passed when I had the pleasure of visiting this interesting locality.

We will return from the under-cliff by the same way we entered; and when again on the high ground, we will turn our faces towards Dovor, and make our way to Shakspeare's Cliff. In the corn-fields about here, the wild Oat (*Avena fatua*), abounds, and grows to a very large size. It is a tall and handsome grass, holding its head with pride over the corn it grows amongst. One culm which I gathered was more than five feet high, and the thickest part of it was upwards of a quarter of an inch in diameter. We all associate the idea of Samphire with Shakspeare's Cliff, from the celebrated lines in *King Lear*. The plant still grows there in profusion, and the samphire-gatherer carries on his adventurous trade of culling the plant from the surface of the cliff, letting himself down by a rope, which passes over a post, holding one nd in his hands, and having the other tied round his waist. As we descend from the summit of this cliff, on the Dovor side, we shall observe Dyers' Green-weed (*Genista tinctoria*) growing; and near the bottom of the descent, in the hedges and on the slopes near the beach, we shall find common Alexanders (*Smyrnium olusatrum.*)

Our rambles in the immediate vicinity of Dovor will conclude here; but I shall, at a future opportunity, request the reader to accompany me in another ramble to Sandwich, where I think we shall find a very ample remuneration for our trouble. Wishing, however, to give as much information as I can on the localities of the plants growing near Dovor, a friend, who collected there this spring, has kindly penned the following observations for me. Most of the specimens I have examined, to ascertain their accuracy, and I think the localities may be depended on.

Marsh Speedwell (*Veronica scutellata*), moist places, East-Wear Bay; wild English Clary, or Sage (*Salvia verbenaca*), under Shakspeare's Cliff; opposite-leaved Pond-weed (*Potamogeton densus*,) water in the lane, near Charlton, parallel with the London-road; Corn Gromwell (*Lithospermum arvense*), close to the London-road, beyond Ewell; common Henbane (*Hyoscyamus niger*,) among the shingles on the edge of the walk, along the Marine Parade, on the rope-walk, near Shakspeare's Cliff, and again by the Martello towers at Folkstone; Hairy Violet (*Viola hirta*,) lanes near Guston; common Herb-Paris (*Paris quadrifolia*,) in a shady lane, about half a mile from the entrance to Lord Guildford's park, on the Dovor side; also abundantly where the wood has been cut down in the same neighbourhood; tuberous Moschatell (*Adoxa moschatellina*), shady lane, just opposite Kearsney Abbey, near River; Sea Campion (*Silene maritima*), frequent on the coast; common Columbine (*Aquilegia vulgaris*,) on the confines of a retired wood, about half a mile from the west side of the London-road, beyond Ewell, near Lydden; common Bugle, white variety, lane near Church Hogham; yellow Weasel-snout (*Galeobdolon luteum*), in shady lanes, near Lord Guildford's Park, also in those beyond River; Hairy Rock-cress (*Arabis hirsuta*), on the sloping banks as you ascend to the castle; tuberous Orobus (*Orobus tuberosus*), on the banks of a field, about a mile beyond Kearsney Abbey; Spring Vetch (*Vicia lathyroides*,) dry banks on the London-road, and close to the sea-shore at Folkstone; Green-winged Meadow Orchis (*Orchis Morio*), in the meadow above the cliff, about half a mile before you reach Folkstone; early purple Orchis (*Orchis mascula*), in the woods and hedges in every direction, particularly fine in the woods at Waldershare; great brown-winged Orchis (*Orchis fusca*), on a steep chalky hill, leading from Lydden to Waldershare; Spider Ophrys (*Ophrys aranifera*), abundant in East-Wear Bay, on the under-cliff and close to the shingles.

W. W. S.

PREVENTION OF EXPLOSION IN STEAM-BOILERS.

ANALYSIS AND CURSORY EXAMINATION OF A REPORT, MADE IN CONSEQUENCE OF AN INQUIRY INTO THE CAUSES AND MEANS OF PREVENTION OF THE EXPLOSIONS OF STEAM-BOILERS; RECENTLY TRANSMITTED TO THE GOVERNMENT OF THE UNITED STATES, BY THE FRANKLIN INSTITUTE OF PENNSYLVANIA.

PUBLIC attention, in the United States, having been roused in the most painful manner, to the disastrous consequences of the frequent explosions of engine-boilers on board steam-boats in that country, the managers of the Franklin Institute of Pennsylvania determined upon an inquiry into the causes of these dreadful events. Such a philanthropic determination was in perfect harmony with the spirit and conduct of the great man, whose name they have adopted as a distinctive appellation; and if they have not hitherto perfectly succeeded in developing every point of this intricate inquiry, they have the distinguished honour of being the first public or private body which has attempted an examination of the subject, by experiments and apparatus on a large scale, designed on purpose to bring the more important current opinions and theories to the test of actual proof. We think that even their omissions, failures, and mistakes, may be of benefit; they may excite further examination, provoke discussion, and tempt new trains of investigation. The disagreement also of their results, with those of some eminent European experimentalists who have preceded them, will impose the duty of a re-examination upon the latter, in those instances, at least, to which the members of the Franklin Institute seem, on their part, to have bestowed the most scrupulous attention.

In a country which, from its hydrographical advantages, is so deeply interested in the extension and prosperity of steam-navigation, it was remarkable, that an inquiry of this importance, rendered imperiously necessary by the repeated destruction of life and property all over the Union, should have been delayed so long. The general government certainly ought to have instituted, much earlier, a minute examination into all the causes of these murderous accidents, and immediately prevented their recurrence, as far as police-regulations could have been effective. They ought then to have gone further, and obtained a *carte blanche* upon the treasury, to enable them to invite the assistance of the most eminent scientific and practical men, European as well as American, and to direct them to examine the evil in every form, and never to relax their labours until they had discovered such specific means of prevention as would render the most colossal steam-generator as innocuous as a tea-urn. They should not have permitted a self-sustained society to drain its little exchequer, and to call upon a few able men among its members, to devote their leisure hours or sacrifice their professional ones, to undertake a work, which to be effective must necessarily be long, laborious, and costly; demanding considerable scientific attainment, great practical skill, and unremitting attention.

It is not too much to say, that to have completely accomplished this object, would, in the present rapidly and widely-spreading extension of steam-navigation, have been a benefit to the whole human race. It would certainly have conferred a reputation of disinterested pursuit of principle to the United States, which they have been reproached with neglecting, and which every admirer of their free institutions would wish them to enjoy. President Jackson and some of his fellow-citizens "down East," may be surprised when they hear it asserted, that the extinction of the national debt might have been honourably delayed a few days for such a noble purpose.

We may be reminded that in an earlier part of this Magazine*, we cited the conduct of the United States' government in this very matter, as one deserving of honourable mention: we had then seen the introductory part only of the Report under consideration. We now see in the *preface* to this Report, (which the committee have, oddly enough, thought proper to *append* to it,) that it was not until after the resolution had been moved and passed by the managers of the Franklin Institute, and while their committee were engaged in the inquiries, that a letter was received from the secretary of the treasury, inviting experiments on the subject of explosions of steam-boilers, at the expense of the treasury department, the house of representatives having placed funds for this purpose at the disposal of the department. But "better late than never," and we believe the general government of the United States has yet the undisputed honour of being first (as a government) in this important examination. Who will *be the second?* We know who ought to be, and to whom it is a disgrace that they were not foremost to discuss and to attain this important desideratum,—we mean the British government. They cannot plead the fear of a want of funds, unexampled opportunities for actual observation and experiment are within their command,—and there is close at hand, to advise and to assist, the Institution of Civil Engineers of London, an association of men who, from their daily pursuits, their peculiar position, their varied intelligence, and their practical experience, would probably be superior for this purpose to any other body in existence.

We proceed to describe what was attempted by the Franklin Institute. On the 10th of June, 1830, the managers appointed a committee of seventeen members, "to examine into the causes of the explosions of the boilers used on board steam-boats, and to devise the most effectual means of preventing the accidents, or of diminishing the extent of their injurious effects." The names of these seventeen gentlemen are given, but, (except in the case of four M.D.s,) their several professions, employments, or pursuits, and their qualifications for this particular purpose, are not mentioned. The only person† whose scientific reputation has been sufficiently extensive to reach Europe, was lately in this country, and openly stated that he never attended a single experiment.

The committee, soon after their appointment, took a proper step to obtain the most valuable kind of information. They addressed a circular to such persons as they supposed were acquainted with the particulars of such explosions, as were up to that period undescribed; and though they published the knowledge they so obtained in the

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* Vol. I., p. 341.

† Robert Hare, *M.D., Professor of* Chemistry in the University of Philadelphia.

Journal of the Franklin Institute, in the years 1831-32, it would have been gratifying to have found a general view of the facts in the introductory part of the Report.

The committee also drew up a series of questions, a plan of experiment based upon these questions, and an estimate of the cost of the experiments. None of these, however, are given in the Report, nor appended to it, omissions which are much to be regretted.

The secretary of the treasury, to whom the questions, plan, and estimate were submitted, approved them, and left the committee free to add any other subjects, or prosecute any other modes of inquiry which might suggest themselves, limiting, however, the sum to be expended. We are certainly curious to know what was the amount of the funds placed by the house of representatives at the disposal of the treasury, for such an object of national interest, and also to know the limit of expenditure prescribed by the secretary of the treasury. These sums, though several times spoken of, are never actually stated in dollars; and we confess we have our fears as to their being of an amount either adequate to the object, or worthy of a great nation. If these amounts should ever be given, perhaps the Franklin Institute will add that of their actual expenditure: it could not fail of being useful.

On the 1st of November, 1830, a sub-committee of seven was appointed to conduct the experiments; these were Messrs. Bache, Reeves, Keating, Baldwin, Merrick, and Lukens. Subsequently the experiments which related to the strength of iron and copper used for steam-boilers, were separated from the others, and confided, on the 4th January, 1831, to a second sub-committee of three persons, viz., Messrs. Bache, Reeves, and Johnson: two of these, it will be observed, were also members of the first sub-committee.

At the end of five years from the date of their appointment, the first sub-committee made a report of their experimental investigations. This, after having been adopted by the general committee, was on the 23rd December, last, ordered by the manager of the Institute to be submitted to the secretary of the treasury, and it has since been published in five numbers of the journal of the society.

The second sub-committee have not, according to the latest accounts received, presented any report; nor have the general committee (as far as we know), made any communication to the public on the subject for which they were appointed, beyond what we have stated.

The omission which we have noticed, of the plan of experiments &c., has produced an apparent want of connexion throughout the report as published; and this, in addition to the occasional absence of perspicuity in the descriptions, and of important detail in the experiments, is extremely annoying and embarrassing to those who are anxious to understand and appreciate, not only that which has been done, but also (what is highly important) the means by which it was accomplished.

Great care and attention appear to have been bestowed upon the construction of the apparatus designed and used for the experiments. Many of the provisions against error, suggested by able experimenters *in analogous* cases, were known and adopted. There was, however, *scarcely any novelty* in the apparatus, nor in its application; probably none was felt to be necessary. To furnish some notion of the size of the apparatus, it may be right to say, that the experimental boiler was a cylinder with flat ends, of rolled-iron, ¼ inch thick. The internal dimensions were, length, 30½ in.; diameter, 12 in.: a glazed aperture was provided in each end for the inspection of the interior.

The subjects proposed to be investigated were arranged under twelve heads. These we shall insert verbatim, and endeavour to give an abstract of the results reported to have been obtained under each; accompanied by a few remarks as we proceed; but reserving for the present, on account of room, a more minute examination and discussion.

Subj. I.—*To ascertain whether, on relieving water, heated to, or above, the boiling-point, from pressure, any commotion is produced in the fluid.*

No experiment is recorded of relieving from pressure water heated to the boiling-point only. A commotion, rendered visible by foaming of a greater or less extent, was observed to attend every sudden diminution of pressure. The amount both of the commotion and the foaming were in proportion to the size of the relieving opening. The increased volume and agitation thus produced, frequently raised the water to the upper part of the boiler, and in some instances discharged both it and steam violently through an aperture made in the top. This effect is, therefore, produced, in a greater or less degree, every time that steam is taken, or let off, from the boiler, whether to supply the engine, to test the water-level, or to reduce the pressure: but it was demonstrated, that although the swelling and agitation of the water produced by a pressure-relieving opening, must often have thrown the water against a larger, and probably a hotter, surface, yet that when such an opening was made, it was uniformly followed by a diminution in the pressure of the enclosed steam. Such reliefs and their immediate effects, consequently, could not be regarded as causes of rupture. It was also shown, that at such times the ordinary apparatus (gauge-cocks), designed to show the height of the water-surface in the boiler were rendered useless. Though several other kinds were tried and condemned, mention is not made of any which were found to be free from objection.

Subj. II.—*To repeat the experiments of Klaproth, relating to the conversion of water into steam by highly-heated metal; and to make others calculated to show whether, under any circumstances, intensely-heated metal can produce, suddenly, great quantities of highly-elastic steam.*

No notice at all is taken of the experiments of Klaproth under this head. A short series of experiments relating to the latter part only of the proposition is given;—the blowing-out of one of the little windows of the boiler, put an end to it at a time when it was becoming interesting. We regret the experiments were not repeated and carried further. The conclusion drawn from those that were made is favourable to the opinion, that great quantities of highly-elastic steam may be suddenly evolved by throwing water upon metal heated to redness. An experiment of Mr. Perkins is alluded to, but not specifically pointed out.

Subj. III—*To ascertain whether intensely-heated and unsaturated steam can, by the projection of water into it, produce highly-elastic vapour.*

The "unsaturated steam" was obtained "by filling the boiler about half full of water, and applying heat below to raise the water to any required temperature; the upper half of the boiler would be filled with steam of an elasticity due to that temperature, this elasticity being measured by the gauge. Fire being now placed upon the top would heat the metal of the upper half of the boiler; and this, by communicating its heat to the steam, would surcharge the latter*."

The result of this investigation we give in the words of the sub-committee. "In no case was an increase of elasticity produced by injecting water into hot and unsaturated steam, but the reverse; and in general, that the greater the quantity of water thus introduced, the more considerable was the diminution in the elasticity of the steam." This supposition of the contrary, they state to be the basis of the theory of the explosion of steam-boilers of their countryman, Perkins. They appear to feel some satisfaction in the idea that they have demolished it. It was not, however, accomplished without some inconvenience; for the sub-committee, "being unwilling to incur any considerable expenditure in this branch of their inquiry, the experiments were rendered *uncomfortable*† beyond anything which occurred in their other researches!"

It is scarcely credible, but the temperature of the water in the boiler beneath the steam was not thought sufficiently important to be noted.

SUBJ. IV.—*When steam, surcharged with heat, is produced in a boiler, and is in contact with water, does it remain surcharged, or change its density and temperature?*

This is subsequently restated thus:—

When steam, surcharged with heat, is produced within a boiler by the contact with heated metal, does this steam remain surcharged, or does it take up water from contact with that in the boiler, and become saturated steam? If the latter supposition be correct, at what pressure and temperature, with regard to the temperature of the surcharged steam, and to that of the water on which it rests?

After this parade of stating and restating, the sub-committe did not make one experiment on this subject. They decide upon the uncomfortable experiments of the preceding investigation, tainted as they then were by the capital omission of the condition of the water, and absolutely vitiated by it for any conclusion on this subject. They rather boldly state that there is nothing to warrant the belief, that with steam and water, under these circumstances, there is any condensation of the former by the latter.

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* To assist those readers who may not see, at once, how that which is surcharged can at the same time be unsaturated, we give an explanation of what is meant by "surcharged steam," "unsaturated steam," so frequently used in this report, (without any reason, but on the contrary) to express the same substance under the same circumstances. We, however, decline attempting to give precision to such expressions as "hot fire," "hot steam," &c., which also occur.

Steam is said to be saturated, (*i. e. with water*), when (A), supposing it to be in a vessel agitated with water, and the temperature of both to be maintained constant, no further evolution of water into steam takes place, however long these circumstances may be continued; or (B), if on the smallest diminution of the temperature *of a volume of steam*, under any circumstances, *a deposition* of water ensues. *Steam is said* to be unsaturated (C), if in the conditions of case A, above stated, water continues to be evolved; or (D) if, in those of case B, its temperature may be reduced without deposition.

Steam is said to be surcharged (*i. e. with heat*) when in the cases C and D above; for it then contains more heat than is necessary to hold the vaporized water in a state of steam, and which heat will unite with other water if it be exposed to it.

Steam is not considered surcharged when in the cases A and B above, for every particle of heat it then contains, is holding a particle of water in a state of steam; and if a particle of heat be abstracted, its co-particle of water will be set free, and a certain volume of steam annihilated.

† As we would not wantonly be the means of making this sub-committee *uncomfortable* again, we acknowledge that we have put this word in Italics in the quotation.

SUBJ. V.—*To test by experiment, the efficacy of plates, &c. of fusible metal, as a means of preventing the undue heating of a boiler, or its contents.*

Considerable pains were taken by the sub-committee on this subject. As no other information on the proportions of the alloys used in fusible metals were in their possession, than those given in Parkes's *Chemical Essays*, they made numerous experiments to ascertain the most useful. Though the sub-committee do not say it expressly, it may be presumed that they consider plates, &c. of fusible metal efficacious for the purpose in view. They consider that the precaution of making the fusible plates, &c. of considerable thickness, as required by the French government in an ordinance issued subsequently to that in which they were first described, would not prove the remedy intended. And then follows this receipt for a true remedy: "The true remedy is to be sought in enclosing the fusible metal in a case, in which it shall not be exposed to the pressure of the steam, but only to its heating effect: the more fluid parts of the metal will not then be exposed to be forced out of the mass; and the whole will become fluid, as if exposed to heat in a crucible."

No doubt! there can be no doubt of this effect occurring,—the most perfect fluidity may certainly be obtained, but *cui bono?*—for what purpose?—the metal is in a case! not exposed to the pressure of the steam! How then is it to act efficaciously as a means of relief to a boiler dangerously increasing in temperature? How is it to act at all, though fluid as in a crucible?

It may become as fluid as "empyreal air" if the boiler will withstand the pressure correspondent to the necessary temperature; but if the boiler yield in the process, will the fluidity of the "true remedy" solidify the eruptive steam? or in what other magical and unknown manner will it cure the prodigious mischiefs which it was the object of all the investigations to prevent? There must be a district of Pennsylvania where the shamrock is worn*!

SUBJ. VI.—*To repeat the experiments of Klaproth, relating to the conversion of water into steam, by highly-heated metal.*

Klaproth is here introduced again, but, as in Subj. II., no further notice is taken of him, nor is any other allusion made to his experiments.

The metals experimented upon by the sub-committee were copper, and iron, both cast and wrought, of various thicknesses, formed into circular bowls, about five and a quarter inches in diameter and three inches deep. The surfaces of the interiors of these bowls were, in some cases, polished, in others, merely smooth, and some were roughened, more or less, by oxydation. The water was first applied in drops, and afterwards in increasing quantities, up to two fluid ounces troy. The experiments were numerous, and the following are the conclusions drawn from them by the sub-committee. We have given them at length, because we intend to return to this part of the subject, as we have grounds for thinking that the sub-committee have been led to place the maxima of vaporization too low.

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* We cannot help thinking that the following parallel case of a "true remedy" would be *prescribed by this sub-committee, if the question were presented to them. Suppose a man has a box which* can only be opened by a certain crooked sixpence; what is the best way of being sure to have the sixpence always at hand when wanted? Answer: Shut it up in the box!—*Enclose the metal in a case!*

"The vaporizing power of copper, when supplied with heat by a bad conductor or circulator, such as oil, increases with great regularity as the temperature increases, up to a certain point, the water being supposed thrown upon the copper surface, in small quantities. Copper flues, heated by air passing through them, would be in this condition if left bare of water, and then suddenly wet. This holds with copper $\frac{1}{16}$th of an inch thick, without indication that a limit will be attained by a much more considerable thickness. The temperature at which the metal will have the greatest vaporizing power, is about 570° Fahr., or about 230° below redness, according to Daniell.

"The law of vaporization of small quantities of water, by a given thickness of copper, is represented with singular closeness by an ellipse, of which the temperatures represent the abscissæ, and the times of vaporization the difference between a constant quantity and the ordinates.

"The same power in thin iron, .04 ($\frac{7}{32}$) inch thick, increased regularly, and was at a maximum, probably, at 510°. With thicker metal the power increases more rapidly at the lower temperatures, and varies very little, comparatively, above 380°, with thicknesses exceeding $\frac{1}{8}$th, and less than $\frac{1}{4}$th of an inch; attaining a maximum at about 507° Fahr., when the quantities are small; rising to 550°, and much above, as the quantity of water is increased relatively to the surface of the metal which is exposed. Quadrupling the quantity of water, the entire amount being still small, nearly tripled the time of vaporization at the maximum.

"When copper of $\frac{1}{16}$th of an inch in thickness, was supplied with heat by melted tin, a worse conductor, and having a lower specific heat than copper itself, the time of vaporization, in a spherical bowl, of quantities varying from $\frac{1}{16}$th to $\frac{1}{2}$ of the entire capacity of the bowl, increased but three-fold, and the temperature of greatest evaporation was raised but 56°, or from 470° to 526°. When the bowl had half of the portion which was exposed to heat filled, the weight of the water was about one and one-tenth of that of the metal.

"The times of vaporization of different quantities of water, varying from $\frac{1}{8}$th of an ounce to two ounces, in an iron bowl $\frac{1}{4}$th of an inch thick, and supplied with heat by the tin-bath, were sensibly, as the square roots of the quantities, at the temperatures of maximum vaporization for each quantity.

"These temperatures were raised from about 460° to 600°, by increasing the weight of water about sixteen times, indicating that considerable quantities of water, thrown upon heated metal, will be most rapidly vaporized when the metal is at least 200° below a red heat.

"While a red heat, visible in daylight, given to a metal, even when very thick, and supplied by heat from a glowing charcoal fire, does not prevent water, when thrown in considerable quantities, from cooling it down so as to vaporize the water very rapidly, it is much above the temperature at which the water thrown upon the metal will be most rapidly evaporated. Thus one ounce of water was vaporized in 13 seconds, at about 550°, in a wrought-iron *bowl $\frac{1}{4}$ of an inch thick*, and required 115 seconds to vaporize in a cast-iron *bowl $\frac{1}{2}$ an inch thick*, at a red heat. Four ounces in the latter bowl vaporized n about 300 seconds, the bowl being red-hot when it was introduced; and two ounces vaporized in 34 seconds at 600° Fahr.

"The temperature of greatest vaporization, with a given thickness of metal, is lower in copper than in iron, the repulsive force being developed at a lower temperature. With equal thicknesses of iron and copper, the vaporizing power of the latter metal, at its maximum was, with the oil-bath, one-third greater than that of the former, and with the tin-bath the power of copper .07 of an inch thick, was equal, nearly, to that of iron, ¼th of an inch thick, each being taken at its maximum of vaporization, for the different quantities of fluid employed. As the maxima for the iron are higher than those for the copper, the advantage will be still greater in favour of copper when the two metals are at equal temperatures.

"The general effect of roughness of surface is to raise the temperature at which the maximum vaporization occurs, and to diminish the time of vaporization of a given quantity of water at an assumed temperature below the maximum."

Subj. VII.—*To determine by actual experiment whether any permanently elastic fluids are produced within a boiler, when the metal becomes intensely heated.*

The experiments made upon this important part of the inquiry are considered as satisfactorily demonstrating that the water in a steam-boiler, in contact with heated iron, whose surface is in the ordinary state clean, but not bright, is not decomposed by the metal.

This is another conclusion in direct opposition to the doctrine Mr. Perkins has been teaching for years. We shall be glad to hear whether he acknowledges or disputes this opinion, after he has examined the experiments and the reasoning of his countrymen.

It is true, that in some of these experiments nitrogen gas, with a variable quantity of oxygen, was obtained from the boiler; and in others, carburetted hydrogen made its appearance: but, however, both of these were traced to their sources,—the first was furnished by some atmospheric air which had leaked into the boiler,—and the second proceeded from the decomposition of the packing of the hand-hole.

Subj. VIII.—*To observe accurately the sort of bursting produced by a gradual increase of pressure within cylinders of iron and copper.*

Two experiments only were made on this subject, but these were considered by the sub-committee to have furnished such ample and correct means of observation, that it was not deemed necessary to institute any other, "especially as they were tedious, and not without danger!"

Two cylinders, one of iron and the other of copper, partially filled with water, were exposed to heat. This was urged and continued, in both cases, until the cylinders burst suddenly with noise, the force of the escaping steam being great enough to drive them from their position to a considerable distance, and scattering the fuel, &c. in all directions.

The sub-committee were of opinion, that these effects were directly conclusive, and showed that "all the circumstances attending the most violent explosions may occur without a sudden increase of pressure within a boiler."

Subj. IX.—To repeat Perkins's experiments, and ascertain whether the epulsion, stated by him to exist between the particles of intensely-heated iron *and water, be general; and to measure, if possible, the extent of this repulsion, with a view to determine the influence it may have on safety-valves.*

The only experiment of Mr. Perkins's which was attempted to be repeated, and that unsuccessfully, was the one in which an opening having been made in one of his generators, containing intensely-heated water in contact with red-hot metal, neither steam nor water escaped, and in which, having affixed a pipe and stop-cock to the same vessel, no steam issued through the cock when opened.

Terrified by the explosion of a wrought-iron mercury-bottle in a stone-quarry, the sub-committee were content with the following safer experiments; but these are interesting, and we give them as reported. As far as they were carried, they certainly support the assertions of Mr. Perkins.

" An iron bowl, about $\frac{1}{18}$th of an inch thick, and having the bottom perforated with small holes, was heated to redness over a charcoal fire, and water poured into it; the mass of metal being small, was cooled down very rapidy to a temperature below redness, and the repulsion which was at first manifested between the water and iron ceased, and the water flowed rapidly through the apertures. Two thicker bowls were provided, one of wrought-iron, $\frac{3}{8}$ths of an inch thick, and the other of cast-iron, seven-sixteenths of an inch thick; the bottom of each was perforated with holes, about .04 of an inch in diameter. When placed over a charcoal fire and heated to redness, water poured in so as to fill the bowls, reduced the temperature of the wrought-iron one most rapidly; but until the reduction was effected, the results were the same as those for the cast-iron bowl. In this latter the water rested upon the bottom without passing through the holes, either as water or as steam; steam formed slowly and escaped from the upper surface, the whole fluid being at a temperature below the boiling-point. The openings were distinctly to be seen, and appeared by measurement to have contracted about $\frac{1}{7}$th part of their diameter; but the repulsion was such as to render the escape of the water quite as difficult, and indeed more difficult, than that of mercury at ordinary temperatures. Removing the vessel from the fire, the water remaining in it, as the material cooled below redness, small particles of water came through at intervals; at a lower temperature large drops collected, which finally united into a full stream. Some rude measurements of the quantity of water which came through when the iron was heated in water at different temperatures, showed a striking diminution at the higher temperatures. These results were obviously not produced by the closing of the apertures as the bowl expanded by heat, the openings being distinctly visible at a red heat.

"The measurements referred to above were as follows: at 58°, $3\frac{5}{8}$ fluid ounces of water passed through the holes, in the cast-iron bowl above referred to, in thirty seconds; the whole quantity of water added, being four ounces.

"In another experiment at 60°, $3\frac{3}{4}$ fluid oz. passed. Water, at between 58° and 60° being thrown into a bowl, previously heated to 82°, $3\frac{1}{4}$ fluid ozs. passed through; when heated to 170°, $2\frac{7}{8}$ oz. passed in the same time, and when heated to 660° about $2\frac{1}{18}$ oz.

"In another series, the same bowl being heated to redness, four fluid ounces of water thrown in were perfectly repelled for fifteen seconds, and at the end of half a minute only $\frac{3}{8}$ths of an ounce had flowed through the openings.

" Of a second quantity of four ounces thrown into the bowl thus cooled, *$1\frac{5}{32}$ oz. passed in thirty* seconds, next $2\frac{1}{4}$ oz. in the same time.

"*These experiments* show that the amount of the force of repulsion between water and heated metal, is measurable even at moderate temperatures, and rapidly increases with the increase of temperature of the metal; the temperature of the water being, in each case of the last set of experiments, nearly the same. They confirm, in this respect, the results of the vaporization of water by metal at different temperatures.

"The pressure of the column of water which was supported over the lowest of these openings at a temperature between 660° and a red heat, or 800°, was less than one inch and a half of water."

We find no observation upon the "influence this species of repulsion may have on safety-valves."

SUBJ. X.—*To ascertain whether cases may really occur where the safety-valve, loaded with a certain weight, remains stationary, while the confined steam acquires a higher elastic force than that which from calculation would appear necessary to raise the valve.*

The disk-valve only, and of one size only, was subjected to experiment. The diameter of this and of its seat are given, but not of the aperture*, which it closed. So far as we can draw any conclusion (for the sub-committee themselves draw none), from the experiments detailed, relating to the subject under investigation, no case of the kind referred to occurred; on the contrary, the valve observed was never stationary, but uniformly rose with a less weight upon it than that given by calculation.

We think that the sub-committee have scarcely touched this most interesting part of the subject. We wish they had exhausted it; for if the case stated in the proposition does ever occur, the conditions of it are certainly unknown, and the term safety-valve ought not at present to be applied to this apparatus. It is clearly a case of misnomer, and may be deceptive to an extent of danger not yet appreciated.

SUBJ. XI.—*To ascertain, by direct experiment, the effect of deposits in boilers.*

The sub-committee made no experiments on this subject. They give a few generalizations, deduced from the examination of some deposits of steam-boat-boilers, but they contain nothing new; and the sub-committee suggest no means of preventing, or of curing, the mischiefs produced.

SUBJ. XII.—*On the elastic force of steam on working pressures.*

The highest reputed working-pressure of high-pressure engines in the United States, is eleven atmospheres. The sub-committee succeeded in experimenting up to ten.

As the sub-committee had designed and constructed that part of the apparatus which was used on this subject of experiment, with the greatest care, they were surprised to find a considerable discrepancy between their results and those obtained and published by the Institute of France. As the difference was too considerable to be admitted as within the limit of error in the apparatus or in observation, the sub-committee, feeling the great weight of such an opposing authority, were led to re-examine their apparatus, their mode of observation, and their results, *very closely*. *After* a severe scrutiny, the sub-committee found no reason to suspect any mistake on their part; and the discrepancy between the observations on the temperature and corresponding pressure of steam, made by the Franklin Institute of Pennsylvania and the Academy of Sciences of Paris, remains to be examined, and the true relation to be pointed out by some future experimenters.

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* *Unless in Pennsylvania* "valve-seat" means "aperture."

The following table contains the results of the labours of the sub-committee of the Franklin Institute; annexed are those of the French Academicians. The fourth column shows the difference between them; the temperatures of the Philadelphians being always lower than those of the French:—

TABLE OF PRESSURES AND CORRESPONDING TENSIONS OF STEAM.

Pressure in Atmosphere.	Franklin Institute.	French Academy.	Difference.	Pressure in Atmosphere.	Franklin Institute	French Academy.	Difference.
	Fahr. °	Fahr. °	Fahr. °		Fahr. °	Fahr. °	Fahr. °
1	212			6	315·5	320·4	4·9
1½	235			6½	321		
2	250	250·5	0·5	7	326	331·7	5·7
2½	264			7½	331		
3	275	275·2	0·2	8	336	342·0	6·0
3½	284			8½	340·5		
4	291·5	293·7	2·2	9	345	350·8	5·8
4½	298·5			9½	349		
5	304·5	308·5	4·3	10	352½	358·9	6·4
5½	310						

The sub-committee conclude this subject by observing, "That while the differences in the results of experimenters are greater than the present state of experimental science warrants, yet at pressures even exceeding ordinary working-pressures, the relation of the temperature and pressure of steam may be considered, in a practical point of view, as sufficiently determined."

We cannot, in justice, conclude our abstract of this report, without expressing a high sense of the labour and perseverance which are displayed in it; and though open to the remarks which we have freely made, it is a valuable specimen of the services that may be rendered to practical science, by associations like the Franklin Institute, when the love of the subject is really existing among the members.

We must also say to the general government of the United States, that they must not consider that they achieved anything which shall acquit them of all further contribution to this, and many other momentous subjects of scientific inquiry, merely because they have simply aided a most meritorious society of their countrymen in defraying some of the expenses incurred. They are in a position, and fortunately it is their interest, to do a great deal more; and we request our Trans-atlantic brethren to lose no time in agitating, not only the general government, but also every one of the state governments, until each is doing something for the great cause.

We now wait for the experiments, &c., of the sub-committee, on the strength of iron and copper used for steam-boilers; and then we shall be anxious for the report of the general committee. The latter will, of course, embrace the whole subject for which they were appointed in 1830.

ON EVAPORATION, VAPORIZATION, AND CONDENSATION.

IMMENSE advantages result from a diligent and an habitual observance of natural phenomena. The vigorous exercise imposed on the mind by the investigation of the laws relating to cause and effect in the varied operations of inanimate nature, insensibly prepares it for more laborious studies, whether in physical or moral science. And, let it be remembered, that the culture here hinted at, is indispensably requisite to the attainment of every species of really useful knowledge. There is no method of acquiring sound scientific information, without thought and persevering attention on the part of the student; and there is no other than *sound* information which can be useful, either as a discipline and high accomplishment of the mind, or as practically applicable in the arts. The business of philosophy is with the *understanding*. That knowledge is falsely and meretriciously called *scientific* knowledge, which is *intended* for the memory, and takes its standing *there* exclusively; and which, consisting of no real acquirements in *any* science, is commonly accompanied by great presumption in all*.

Among the numberless processes which are the offspring of human contrivance and ingenuity, those only can be successfully and economically conducted, whose conditions are conformable to the laws on which the constitution and preservation of our globe depends. To alter or subvert any of those laws is no part of man's prerogative,—as well may he attempt to create a world. The immutable principles, whose increasing energy is essential to the existence of matter in all its diversity of forms—producing in it the successive changes of which we are at the same time the subjects and the witnesses—are, perhaps, few in number and extremely simple in their modes of operation; or, it may be, that whilst their number exceeds the powers of a finite being to calculate, so it is possible that their reciprocal actions on the elements of nature are of so refined and recondite a character, as for ever to elude the grasp of the most highly-endowed mind. But this is a subject on which we have no intention further to speculate.

Of many interesting phenomena, and important properties of matter, it might be said, that the accumulated evidence of ages attests their verity. The true explication or discovery of others, which are no less interesting as objects for contemplation, nor less important as respects their consequences to society, has, however, been reserved for modern times.

The blessings held in trust by philosophy, are eminently adapted to the wants of every nation under heaven; being, in fact, the joint-stock property of the whole family of man. That in by-gone days so much lamentable ignorance prevailed, even in countries whose boast has been of civilization and refinement, was not so much the fault of the people as of their rulers. The time was, that intellectual superiority subjected its possessor to persecution and cruel punishment; physical science was viewed as synonymous with satanic influence; and he who dared to be exposed, water exhibits a tendency to assume the vaporous form; and not only so long as it retains its fluidity, but also when in a solid state. Almost every other liquid is liable to spontaneous evaporation at common temperatures. A few solid bodies are also susceptible of it, among which camphor might be mentioned as a striking example. It is probable, from many concurrent circumstances, that a property common to all bodies, whether solid or liquid, is thus to diffuse their particles in the atmosphere, although in some instances, the process might be so extremely slow as to be wholly inappreciable.

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* *Rev. H. Moseley, on Mechanics applied to the Arts*, p. xxxvii.

The temperature at which a liquid boils, termed its *boiling-point*, and of which we shall treat more particularly in a future paper*, materially affects its rate of evaporation. Thus, when equal quantities of ether (whose boiling-point is 96°), of alcohol (173°), and of water, (212°), are exposed to the air, under precisely similar circumstances, the first that disappears is the ether, the alcohol next, and a considerable time afterwards the water. Hence, we learn, that the liquids whose boiling-points are lowest, are dissipated with the greatest celerity.

Evaporation takes place only at the *surface* of liquids; extent of surface, therefore, all other conditions being equal, will necessarily influence the rate of evaporation in similar or dissimilar liquids. This process is likewise affected by the temperature of the air in contact with the surface of liquids, by its being dry or moist, in motion or at rest. The density, or pressure, of the atmosphere, determines the rate of evaporation, as it does also the boiling-point of liquids. This, however, belongs to another part of our subject.

Equal quantities of water, in similarly-formed vessels, being exposed to air of very different temperatures,—say 35° and 85°,—that in the warm situation will disappear before the other will have sustained any sensible diminution. When air next above the surface of a liquid is charged with moisture, evaporation proceeds but very slowly, although it might happen that the temperature of the air is higher than that of the liquid. Air at rest is a condition unfavourable to evaporation, but when in rapid motion, whatever might be its temperature, it is the most active agent in spontaneously promoting it with which we are acquainted. The effect of what is termed a *drying-wind* on a cold day, is somewhat surprising. A few hours suffice for its removing every symptom of recent rain; roads which were previously covered ankle-deep with dirt, become dry and hard; pools of water disappear; and the whole face of the country wears a totally different aspect.

The vapour, or as it is commonly termed, *steam*, of boiling water, when in a perfect state, that is, so long as it retains the temperature (212°) of the water from which it is generated, is *dry* and *invisible*; a description that applies equally to steam, (aqueous vapour,) at any temperature during its union with atmospheric air. When vapour loses its transparency, it is a proof of partial condensation.

On observing attentively the steam that escapes from the spout of a tea-kettle, at the moment the water begins to boil, we shall perceive *the former* to be in immediate contact with the spout, on the interior edges of which, a portion of it will be condensed in minute drops. A few moments afterwards, provided the water continues to boil, the spout of the kettle will become perfectly dry, and, at the same time, in its immediate vicinity, there will be a certain space, say from one-half to three-fourths of an inch, throughout which not a particle of steam will be perceptible. It is easy to explain this. When the water in the kettle begins to boil, the spout being at a lower temperature than the steam issuing from it, a portion of that steam is condensed. As more steam escapes, it soon communicates to the metal its own temperature, condensation ceases, and the spout becomes dry. By this time the steam has acquired sufficient force to displace the air immediately opposite the orifice of the spout, whence it issues dry and invisible. As it is cooled by mixing with the surrounding air, it assumes its well-known cloudy appearance. By a further reduction of temperature it becomes again invisible.

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* *We purposely* abstain from entering into minute details here. The announcement *of general principles* will occupy the space allowed us in the present number.

As a necessary result of natural or spontaneous evaporation, air always contains water in a vaporous form, the quantity being subject to perpetual variations, dependent on changes of temperature, and other disturbing causes to which the atmosphere is liable. To some it might be difficult to understand how water and air unite—the heavier body being suspended in the lighter—but there is no one fact in philosophy that admits of more satisfactory proof.

The lower the temperature of air, the smaller is the quantity of aqueous vapour united with it. When air is so fully charged with vapour that it refuses to take up an additional supply, it is said to be *saturated*. In this case, any depression of the temperature of the air is accompanied by the condensation of part of the vapour present in it; whilst, on the contrary, air that is charged with vapour very much below its point of saturation, will suffer a considerable reduction of its temperature before it parts with any of that vapour. When vapour and air are in the act of separating, an elevation of temperature not only interrupts the further progress of condensation, but the partially-condensed vapour immediately resumes its former state of invisibility. The particular temperature at which condensation occurs, is termed the *dew-point*. To determine this with accuracy is of the last importance in making hygrometrical experiments. We shall soon have occasion to treat of these, and also of the various instruments employed in conducting them.

The atmosphere is a vast store-house, whence the solid portions of our globe receive their supplies of moisture. By an increasing process of evaporation, millions of gallons of pure water are raised daily into the air, which, after having been distributed by the united agency of the sun and wind, return in due time, and under various forms, to the surface of the earth.

A few brief allusions to some of the meteorological phenomena, suggested by the foregoing observations, will conclude this paper.

Already has it been noticed, that aqueous vapour shows no disposition to separate from air, so long as the temperature of the latter continues higher than its *point of* saturation. The vapour thus present *in air, although exhibiting no* symptoms of opacity, will, however, readily impart moisture, by its condensation, to the surfaces of surrounding bodies; and to produce this effect, it is only necessary that those bodies should be a few degrees colder than the air with which vapour is united. A simple experiment with a glass of cold water, illustrates this principle, on whose operation depends the formation of *dew*.

At night, when the great source of heat has withdrawn his energies, the earth rapidly cools, and a portion of the vapour in the air which lies next above its surface is deposited thereon. Hence the origin of dew, whose importance is better understood in regions where rain falls only three or four times in a year, than it is in our variable climate.

A reference to that well-known property of heat, termed *radiation*, is sufficient to indicate the means by which, in almost an incredibly short time after the sun has set, the earth acquires a temperature lower than that of the contiguous stratum of air. To what height this radiant heat ascends in the atmosphere, is, of course, dependent on a variety of circumstances, of which our knowledge is exceedingly limited. That the air at a considerable distance above the earth is much colder than in its immediate vicinity, is attested by the observations of those who have ascended in balloons, and traversed lofty mountains, the summits of which, even in tropical climates and beneath cloudless skies, are covered perpetually with snow.

The conditions which are known to favour radiation in bodies, generally exercise precisely the same influence in the formation of dew. For instance, if a plate of polished metal and a piece of dark-coloured cloth of equal sizes are exposed to the atmosphere, under precisely similar circumstances, on a clear night, the metal will maintain the temperature of the air next above it, but the cloth will be many degrees colder; and, whilst the former will remain perfectly dry, the latter will imbibe a profusion of moisture. At the surface of a smooth gravel-walk there will be scarcely any change of temperature during a whole night. On fresh-turned mould, or on grass, a thermometer will show a difference in the former case of 10°, and in the latter of 15°. Hence, it follows, that the most copious deposit of dew will be upon the grass.

That heat passes by radiation from the earth towards a cold stratum of air, at a great elevation, is further illustrated by the effects often produced through the intervention of clouds on a previously clear night, when the deposition of dew will immediately cease, even in the most favourable situations; and not only so, but as will sometimes happen, that which had been deposited will speedily disappear, indicating that the temperature of the earth and of the air in contact with it, are very nearly equal. Whatever, therefore, prevents the escape of heat from the earth retards the deposition of dew; whatever accelerates the cooling process, promotes, in an equal degree, the condensation of aqueous vapour.

A great proportion of the moisture exhaled during the day is evidently designed to refresh vegetation at night. Who can fail to admire the arrangements by which the distribution of this moisture is effected? *Vegetable substances*, and especially blades of grass, radiate heat more *rapidly than other* bodies usually found at the surface of the earth—on vegetables, dew is deposited in the greatest profusion. The fresh-turned mould, into which has been cast the seed, on whose speedy germination the hopes of a future harvest depend, receives also its allotted proportion. The gravelled road—the paved street—the barren mountain—the uncultivated plain—have no need of dew. From them it is, comparatively, withheld.

When air, which is heavily charged with vapour, has its temperature reduced below the point of saturation, either by admixture with air colder than itself, or by an enlargement of volume in its ascent to a greater altitude, a portion of the vapour present in it will be partially condensed. If this occurs to any great extent near the surface of the earth, the partially-condensed vapour, which is believed to consist of inconceivably thin vesicles of water, each separate vesicle enclosing a portion of air, constitutes a *mist* or *fog*. When this partial condensation takes place, as it most commonly does, at considerable elevations in the atmosphere, the accumulated masses of vapour are denominated *clouds*. The vapour of which clouds are composed, is perfectly condensed by a further reduction of temperature, and the water resulting from it unites in drops, which descend in the form of rain. If the condensation proceeds gradually, the rain will fall in a gentle shower; if it be sudden, as when lightning and thunder prevail, it constitutes a storm*.

Snow consists of vesicles of water, as mentioned above in reference to clouds, frozen in the upper regions of the atmosphere. *Hail* is perfectly-condensed vapour, like rain, formed into drops and frozen during its descent. *Hoar-frost* bears the same relation to dew that snow does to rain—the first being partially-condensed vapour, frozen at the surface of the earth; the latter, vapour in a similar state, frozen previous to its descent.

When two or more currents of air, moving in opposite directions, meet, if each current is heavily charged with vapour, it not unfrequently happens that the sky becomes suddenly obscured by dense clouds. This oftener occurs in summer than at any other season, and as clouds formed under such circumstances but rarely yield rain, they prove a source of anxiety, or disappointment, to the husbandman. On some occasions it may be remarked, when two opposing currents of air meet, one of which is warm and charged with vapour, the other cold and comparatively dry, the clouds floating in the warmer current will, in a few minutes, change their appearance, and, finally, vanish away. This is an instance of partially-condensed vapour accommodating itself to a change of circumstances, and uniting with air at a reduced temperature. It sometimes happens, when a shower of rain is falling, that a current of cold dry air will suddenly infringe upon the rain-clouds. In that case rain is changed into hail, an occurrence not uncommon on a warm day in Summer.

By ordinary observers, clouds are viewed only as confused masses of vapour, driven hither and thither at the mercy of the winds. We are assured, however, by those who have had opportunities of closely examining them, that their structure is singularly beautiful; the vesicles of which they are composed being arranged with as much regularity as the fibres of the most delicately-formed flower.

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* *See Magazine of Popular Science,* vol. i., pp. 23—26, for some interesting *particulars respecting Rain.*

As, in evaporation, *heat* performs an important office, so in the diffusion of vapour through the atmosphere, its accumulation in the form of clouds, its dispersion, reunion, and final precipitation upon the earth, the agency of *electricity* is no less conspicuous. Our knowledge of the conditions which determine the developement of electricity is necessarily imperfect. A few of these conditions are tolerably distinct; but others are veiled in uncertainty, simply because our post of observation is remote from the scene of their operations.

The variable distances at which clouds float in the air, obviously implies a peculiarity of texture adapted to their respective situations and circumstances. Hence they admit of a classification which enables those who are skilled in this branch of Meteorology to distinguish by their form, colour, and relative distances, the several varieties, and to predict with considerable accuracy what kind of weather is likely to prevail.

THE BRITISH ASSOCIATION.

The Sixth Meeting of the Association had been last year fixed by the General Committee to be held at Bristol. A variety of considerations influenced the selection of this site from a number of others, whose claims were put forward. The Association had met once in the north of England (at York), twice in the midland counties (at Oxford and Cambridge); it had travelled into Scotland (to Edinburgh); and to hold its last meeting it had gone to Ireland. Thus might it be said to have perambulated every part of the United Kingdom, except the southern part of it: and thus it was that the claims of Bristol, the capital of the south, to the distinction—often and warmly preferred—were, after more of opposition, canvassing, and party feeling, than might have been expected, preferred to those of Liverpool, Manchester, Birmingham, and Newcastle, all of which great cities had, we believe, their deputations present.

The place of meeting had, moreover, advantages peculiar to itself. The north of Somerset is a county of great geological interest, it connects itself by an interesting chain of geological gradations with the much-debated carbonaceous district of Devonshire, and its blue Lias quarries are in themselves a fossil museum. And, moreover, as lovers of science are not unfrequently idle men (with respect be it spoken), and lovers of pleasure, to their investigations were offered the magnificent scenery of the northern coast of Devon—Porlock, Linton, Ilfracombe, and Clovelly—names treasured amongst the tenderest recollections of travellers in search of the picturesque. Who, too, had not heard of that delightful excursion of a day, from Wells to Weston, if he had never been fortunate enough to make it—a day's journey which includes within it a visit to the remarkable cavern of Wokey, the cliffs of Cheddar, the bone-cave, more correctly the hyæna-den, of Banwell, and the bay of Weston. Again, northward, there were the Severn and the Wye, offering themselves to the use of the Association, for an excursion to Chepstow and to Tintern;

and, last, but not least of objects of interest, was Clifton itself, with its vast limestone cliffs, or rather quarries, crowned with terrace upon terrace, and crescent upon crescent, of Bath-stone palaces.

But, to the actual business of the Association. The following is a list of the General Officers, as advertised for the occasion:—

Trustees (Permanent)—C. Babbage, Esq., F.R.S.; R. J. Murchison, F.R.S.; John Taylor, Esq., F. R. S.
President,—The Most Noble the Marquis of Lansdowne.
Vice-Presidents,—The Most Noble the Marquis of Northampton, F. R. S.; Rev. W. D. Conybeare, F.R.S.; James C. Prichard, M.D., F.R.S.
General Secretaries,—Francis Baily, F.R.S.; Rev. William V. Harcourt, F.R.S.
Assistant General Secretary,—Professor Phillips, F.R.S.
Treasurer,—John Taylor, F.R.S.
LOCAL OFFICERS.—*Treasurer*, George Bengough, Esq.
Secretaries, C. Daubeny, M.D., F.R.S.; V. F. Hovenden, Esq.

At each of its meetings, the first step is the assembling of its General Committee, composed of those of its members who have contributed papers to the published transactions of philosophical societies, or who have been appointed deputations from such societies; and this committee met at Bristol, at twelve o'clock on Saturday the 20th, in the Chapter-House of the Cathedral. Among the more distinguished of the members present, we observed Professor Sedgwick, Professor Babbage, Sir David Brewster, Mr. Baily, Mr. Whewell, Sir W. Hamilton, the Provost of Trinity College, Dublin, (Dr. Lloyd,) Dr. Dalton, Professor Moseley, Professor Lloyd, Dr. Daubeny, Dr. Lardner, and Mr. Vernon Harcourt. The chair was taken by Mr. Whewell, the minutes of the proceedings of the committee at its last meeting were read, and the arrangements for its present meeting detailed by the honorary secretary, Mr. Vernon Harcourt, and the assistant secretary, Professor Phillips,—the latter referred principally to the officers recommended to be appointed in the different sections,—their presidents, vice-presidents, secretaries, and committees: they will be found detailed below.

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(*For Geography*) R. I. Murchison, Esq.

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(*For Geography*) F. Harrison Rankin, Esq.

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SECTION F.—STATISTICS.

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SECTION G.—MECHANICAL SCIENCE.

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Vice President—M. I. Brunel, Esq., John Robison Esq.

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Geo. Rennie, Esq.
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Rev. W. Taylor

The scheme of the proceedings of the Association was to remain as heretofore; as, however, some of our readers may not be acquainted with it, and as some knowledge of it will perhaps assist them in following what we may have hereafter to say, we shall here, in a few words, describe it.

The subjects, the discussion of which is considered as properly belonging to the province of this Association, are embraced under seven distinct heads, and the members attaching themselves to the discussion of them, are supposed to divide themselves into seven distinct sections, which are, in their collective capacity, *the Association;* and which are distinguished from one another, and designated by seven successive letters of the alphabet.

To each of these sections are assigned officers—a president, vice-presidents, secretaries, a committee, and a place of meeting. At Bristol, section A met in the Merchants' Hall; section B, at the Grammar-school; section C, at the Institution; sections D and E, at Colston's School; section F, at Harril's Rooms; and section G, at the Merchants' Hall.

The committee of each section assembles at half past ten o'clock in the morning, and the section itself at eleven o'clock.

It is to the Committee of the Section that any paper intended to be discussed in it, is first submitted, and being approved of there, it is announced on the day of meeting, with the other subjects of discussion, in the order in which it will be taken. The business of each section occupies it from eleven to two or three o'clock, when it separates. For the evenings of four alternate days of the week, meetings of the Association are fixed, not, as in the mornings, in sections, but collectively, in some place sufficiently large to contain the whole body; and the ladies who may be disposed to honour the deliberations of the Association with their presence, of whom each member is allowed to introduce one. At this evening-meeting the President of the Association presides, and the other officers and more distinguished members of it are collected round him; and among the rest the presidents of the several sections. These each in his turn then reports to the meeting the proceedings of his section during that and the preceding day, stating concisely the results of the discussions which had been induced, and the more remarkable of the facts elicited; thus, although the attention of each member during the day-time, is of necessity limited to the deliberations of the particular section to which he attaches himself, yet in the evening he is enabled to obtain, at least, some general notion of what has been doing in other sections of the Association.

The reading of these reports of the presidents of sections is followed, on the first evening of meeting, by a general report of the secretary for the year, of the progress of Science during that year—a report usually of great interest and importance, directing the attention of the members to the more recent researches of learned men, and keeping the Association, as it were, continually on a level with the onward progress of Science. On the other evenings of meeting, the reports of the presidents are followed by some lecture from one of the more eloquent of the members appointed by the Council for that purpose; or some discussion, on a subject of interest and importance, among the most distinguished persons present.

Such was the scheme of the proceedings of the Association at its previous meetings, and such was the plan now to be adopted. The places of sectional meetings have been mentioned; the place appointed for the general meetings was the Theatre, of which the president, officers, and general committee of the Association were to occupy the stage, and the other members the pit, gallery, and boxes. The president appointed for the year was the Marquis of Lansdowne; on the morning of Saturday however, a letter was, received from the noble Marquis, announcing that he was prevented from coming to Bristol, by the alarming illness of his eldest son, the Earl of Kerry; and an express was immediately sent off to solicit of the Marquis of Northampton, who had announced his intention of being present, that he would relieve the Association from its present embarrassment, and take upon himself the office of president. The business of the *general committee* possessed little interest, except in the announcement of this fact, the declaration of the officers and committees for the year, of the places of sectional and general meeting, of the place (no unimportant matter) of dining, and of a promenade at the Zoological Gardens and on the evenings at Miller's Nursery-Ground, when there was no general meeting in the Theatre,

There was read the treasurer's report, from which it appeared that the property of the Association, including the estimated value of a number of copies of the printed transactions, was 4564*l.* A committee of recommendations, which had been appointed from time to time, was reappointed: a somewhat unintelligible motion, that the Council should meet during the week, and exercise its proper functions in the constitution of the Association, which it appears had remained in abeyance, was carried, and the General Committee broke up.

The first business of the Association having thus been completed, we went forth from the ancient Chapter-house appointed for its sittings, and found ourselves in a shower of rain: the day was most unpropitious, —it rained in torrents until the evening. Nevertheless, the streets presented the manifest evidence of a new and strange excitement; they were full of passengers, of whom every fourth or fifth might be distinguished to be a philosopher, by the manifest impatience of his motions and frequent and feverish inquiries for some part or another of the great city, where he was to take up his abode. Of all such inquiries, however, the most frequently reiterated was, no doubt, the inquiry for the Inquiry-room, such being the attractive designation assigned for the time to certain auction-rooms in Corn-street, called Harril's Rooms. We ourselves followed the stream to these Inquiry-rooms, rejoicing in the hope of a complete and comprehensive solution of all the difficulties which in the multiplicity and complexity of such manifold plans and arrangements we felt ourselves oppressed with. To our dismay, on arriving at the room of Mr. Harril, we found it swept and garnished; and instead of the smooth and facile suavity, the "frons urbana" of a man, on whom had devolved the responsible but unenviable functions of an answerer of inquiries, we encountered a very abrupt and somewhat ill-mannered person, from whom it was with difficulty that we could learn so much as this, that our steps must be retraced to the Grammar-school, where our inquiries might be repeated. Although thus disappointed, we were yet secretly, and in the pride of our humble philosophy, gratified by this other realization of a principle which we have held from our very school-days, founded, as we believe, in the very nucleus of human nature, and common to all times and ages of men, but generally cited in those words of Horace, "Lucus a non lucendo;" a principle, according to which the true designation of a thing is to be taken as the very opposite of that which is assigned to it. Thus convinced that these rooms were called Inquiry-rooms, because inquiries were not answered there, we found, as we best could, our way to the Grammar-school. Here we received a programme, of which the following is a copy, and renewed our ticket according to the prescribed forms.

PROGRAMME.

At the Inquiry Room, in Harril's Rooms, Corn Street, which will be open on *and after Saturday,* the 20th of August, to Members, and Persons who are desirous of becoming Members, it is intended that information shall be afforded on all matters relating to the admission and location of Members, and the arrangement of business. In this room, Lists of Furnished Lodgings,—Plans of the Tables at the Ordinary,—Notices of Meetings and Excursions,—Titles of Communications appointed to be read in each Section daily,—printed Lists of Members, who have arrived, with their Addresses, in Bristol,—and all other Notices relating to the Association, will be placed in conspicuous situations.

At all other places of meeting, and at the Ordinary, Members will be requested to exhibit their Tickets.

The General Committee will meet in the Chapter Room of the Cathedral, on Saturday, the 20th of August, at Twelve o'Clock; and afterwards according to adjournment. Special notice will be given of the time when the concluding Meeting of this body will be held.

Sectional Meetings. The Sections will assemble every day during the week, Saturday excepted, at Eleven o'Clock, in the following places.

Sections.	Places.	Provisional Secretaries
A *Mathematics and Physics*	...Merchants' Hall ...	Mr. F. W. Jerrard.
B *Chemistry and Mineralogy*	...Grammar School ...	Mr. William Herapath.
C *Geology and Geography*	Institution	Mr. Wm. Sanders, Mr. S. Stutchbury.
D *Zoology and Botany*............	Colston's School......	Dr. Riley, Mr. S. Rootsey.
E *Medical Science*	Colston's School......	Dr. Symonds.
F *Statistics*	Chapter Room	Mr. C. B. Fripp.
G *Mechanical Science*	Merchants'Hall	Mr. T. G. Bunt, Mr. W. West.

The Committee of each Section will meet daily at Ten, A.M. in rooms adjacent to the Sectional Rooms.

At or before Ten o'Clock daily, Lists of the Communications appointed to be read in each Section will be placed in the Inquiry Room, and on the doors of the respective Sectional Rooms.

General Meetings of the Association will be held in the Theatre, King Street, on Monday, Wednesday, and Friday, at Eight, P. M. The concluding Meeting will take place on Saturday, at an hour to be fixed by the General Committee.

On the Evenings of Tuesday and Thursday there will be no formal business; but arrangements will be made for the purpose of affording opportunity for conversation, experiments, and short discourses.

A limited number of Ladies' Tickets will be issued for the Evening Meetings.

An Ordinary for the accommodation of strangers (at 5*s.* per head, not including wine) will be provided daily during the week, at the Rooms of the Horticultural Society, at the upper end of Park Street. Plans of the Tables will be shown daily at the Horticultural Society's Rooms and at the Inquiry Room. Members may select their places for any day, by inscribing their names before Eleven o'Clock of that day, at the Inquiry Room; or in the evening of the previous day, at the Horticultural Society's Rooms.

Members of the Association will be admitted to various public and private establishments, on exhibiting their Tickets.—A printed List of such establishments will be placed in the various Meeting Rooms.

Having now made our inquiries, and it must be admitted satisfied them, we betook ourselves to an hotel in Clifton; and on announcing our intention to sleep there, to our astonishment the communication was met by an inquiry, whether we were or not a member of this Association. On acknowledging, as we are always disposed to do, with much self-complacency, that that distinction had fallen to us, we were informed that it would cost us at least five shillings for a bed, however small the bed-room, and that if our ambition extended beyond a mere closet, ten shillings and sixpence would be the price of a night's repose; moreover, that to sleep as though we were not a member of the Asssociation, would cost for every night of such philosophical slumber, one guinea. Much we had heard in our time of a curious method of sleeping at *Bristol with one eye open,*—and convinced that there was some truth in it,—we determined to sleep, at any rate for this night and the next, unphilosophically and less expensively, in Bath, a scheme favoured by the appearance of a Bath coach at the door.

Ten o'clock on Monday morning brought us back to Bristol, and found the streets of that great city thronged with newly-arrived philosophers, pouring forth from the hotels and lodging-houses, and crowding along Corn-street to the Inquiry-room, where its functions were somewhat more cheerfully exercised than on the preceding Saturday; here, having gone through the form of supposing an inquiry, and an authentication of their qualifications to be admitted members, they were referred to the Council House, called the Reception-room, where they were admitted, and whence each hastened to the particular section in whose deliberations he proposed to take a part.

Notwithstanding the delay occasioned by the extraordinary influx of persons applying all this morning to be enrolled members of the Association, sufficient numbers were, by eleven o'clock, admitted to fill, we believe, the place of meeting of every Section of the Association. On this, however, and on the succeeding days, it appeared to us, that the most crowded of the Sections were those of Geology and Mechanical Science, by reason, perhaps, of the more intelligible and popular character of the subjects discussed in these Sections.

Before the expiration of the first day, 1100 members were enrolled, and, eventually, the number amounted we believe to more than 1300; among whom were the Duke of Beaufort, the Marquis of Northampton, Earl Bathurst, Lord King, Lord Nugent, Lord Sandon, Lord Edward Somerset, Lord Cole, Lord Browne Mill, The Lord Bishop of Bath and Wells, Baron Dupin, The Right Hon. Spring Rice, Chancellor of the Exchequer, Right Hon. Henry Hobhouse, Hon. C. A. Harris, Capt. Sir John Ross, Sir Charles Lemon, Sir Thomas Ackland, Sir H. Strachey, Baronets; Sir David Brewster, Sir David Wilkie, Sir Peter Laurie; T. G. B. Estcourt, M.P.; T. Estcourt, M.P.; J. J. Guest, M.P.; R. B. Hale, M.P.; G. A. Hamilton, M.P.; H. Handley, M.P.; Colonel Gore Langton, M.P.; E. A. Sandford, M.P.; P. J. Miles, M.P.; W. Miles, M.P.. T. Wise, M.P.; Professors Buckland, Daubeny, Rigaud, Powell (Oxford); Sedgwick, Cumming, Henslow, Challis; Rev. W. Whewell, Rev. G. Peacock, (Cambridge); Professors Moseley, Babbage, Bailey, Christie, Cooke, Don, Phillips, Ritchie, Todd, and Wheatstone (London); Forbes, (Edinburgh); Johnston (Durham); Barker, Evanson, Geoghegan, Sir W. R. Hamilton, Lloyd, M'Caul, and M'Culloch (Dublin); Stevelly (Belfast); Von Breda (Leyden,); Moll (Utrecht); Parigot (Brussels); Munier (Geneva); Nilsson, (Lund, Sweden); Von Raumer (Berlin); Hare (Philadelphia); Doctors: Lloyd (Provost of Dublin); Dalton (Manchester); Lardner, Apjohn, Bliss (Oxford); Bowring, Fiske (America); Henry, Luppenburg (Hamburg); Metcalf (Kentucky); Davies Gilbert, Esq., Thomas Moore, Esq., the Very Rev. the Dean of Wells, the Rev. W. L. Bowles, Rev. W. D. Conybeare, &c.

It will, we conceive, be more in the spirit of this journal, and more satisfactory to our readers, to present our account of the proceedings of the Association, not under the form of a daily report, but as a conspectus *of the whole.*

The following, then, are the titles of the papers read, and the communications made, in the different sections during the week.

SECTION A.

Sir David Brewster reported progress as to the experiments directed at the former meeting to be instituted on the construction of a Lens of Rock Salt.

Mr. Lubbock communicated the result of some important Observations on the Tides at the ports of London and Liverpool.

Mr. Whewell reported proceedings of the Committee appointed by the Association to fix the relative value of Land and Sea.

Mr. Lubbock introduced a paper on the formation of an empirical Lunar Theory.

Professor Sir William Hamilton gave an account of Mr. Jerrard's Mathematical Researches connected with the general Solution of Algebraical Equations.

Professor Phillips made a brief statement of the operations of the Committee appointed by the Association for the purpose of making a series of experiments to determine the Subterranean Temperature of the Earth.

Mr. Craig read a paper on the Polarization of Light.

Mr. Russell read an important paper on the phenomena of Waves and Currents.

Professor Powell communicated some observations on Refractive Indices.

Sir David Brewster read a paper on a Singular Developement of Polarzing Structure in the Crystalline Lens of Animals after Death.

The Rev. Mr. M'Cauley read a paper in continuation of one communiated to the Association last year, on the application of Electro-Magnetism o Mechanical Purposes.

Mr. Harris read a paper on some Phenomena of Electrical Repulsion.

Professor Challis made a supplementary Report on the Mathematical Theory of Fluids.

Professor Stevelly made some remarks on the interpretation of the doubtful sign in certain Algebraic Formulæ.

Mr. M'Culloch read a paper on the Laws of Double Refraction of Quartz.

Mr. Adams made a communication on the Interference of Sound.

Mr. Talbot reported his Researches on the Integral Calculus.

Dr. Apjohn read a paper on the Specific Heat of Gases.

Professor Hamilton made a communication on the Calculus of Principal Relations.

The Rev. William Scoresby described two Magnetical Instruments.

Professor Forbes read a paper on the Terrestrial Magnetic Intensity at various Heights.

Sir David Brewster read a paper on the Action of Crystalline Surfaces.

Mr. W. G. Hall made some remarks on the Connexion of Weather with the Tide.

Mr. Ettricke read papers "On an Instrument for observing Terrestrial Magnetism;" "On improved Rubbers for Electrical Machines;" and "On a New Instrument for trying the Effect of Electrical Discharges in rarefied Air, or in different kinds of Gases."

Mr. Addams made some observations on the Vibration of Bells.

Dr. Reutzi introduced a paper "On the Higher Order of Grecian Music;" and another "On Mnenomic Logarithms."

Mr. Whewell read a paper on a New Anemometer.

Professor Phillips read a notice of the probable effects of elevated ground in the direction of the Lines of equal Magnetic Dip.

Sir David Brewster described some valuable Improvements in the Telescope.

Mr. Russell read a paper on certain Elements of the Resistance of Fluids that appear to be internally connected with the application of Analysis.

Dr. Hare made a communication on the Electric Spark.

Dr. Carpenter described a system of teaching the Blind to read.

Mr. Hodgkinson gave an account of some experiments made at the request of the Association, to determine the comparative strength and other properties of iron, made with the Hot and Cold Blast.

Section B.

Mr. Watson communicated the results of experiments on the Pyrophosphate of Soda.

Mr. Ettricke described a new form of Blowpipe.

Mr. Herapath produced an Analysis of the Water of the King's bath, at Bath.

Dr. Hare, of Philadelphia, made an important communication on Rock-Blasting; and also described a Gas-meter which he had for many years found of great use, but which had not yet been used in this country.

Mr. William Herapath gave a short account of the Aurora Borealis of the 18th of November.

Mr. Thomas Edley furnished a paper entitled, "Important Facts obtained Mathematically from Theory; embracing most of those experimental results in Chemistry which are considered as ultimate Facts."

Dr. Charles Henry read a paper on the Power of certain Gases to prevent the union of Oxygen and Hydrogen.

Mr. W. Herapath read a paper on Arsenical Poisons.

Dr. Hare made some observations on the improvements of the Galvanic Pile.

Dr. Daubeny read a report on the present state of our knowledge with regard to Mineral-Waters.

Mr. Mushet exhibited specimens of Iron Ore, and also of an Iron Cement, which he stated to possess superior binding properties.

Professor Johnson explained the constitution and properties of Para Cyanogen.

Mr. W. West read a paper on the Substances diffused through the Atmosphere.

Dr. Hare read a Copy of a correspondence between Berzelius and himself, on Chemical Nomenclature.

Dr. Dalton made some observations on Atomic Symbols.

Professor Johnston brought before the Section his Chemical Tables, of which a specimen, entitled "Chemical Constants," had been laid before the Association in Dublin.

Dr. Thomson read a detailed account of experiments on the Combinations of Sulphuric Acid and Water.

Mr. W. C. Jones read a paper on a peculiar modification of Gluten.

Mr. Crosse described certain improvements in the Voltaic Battery, and also read a paper on Atmospheric Electricity.

Mr. Scanlan gave an account of a new Compound, found during the destructive Distillation of Wood.

Professor Davey described a peculiar compound of Carbon and Potassium, and also a new Gaseous Bicarburet of Hydrogen.

Dr. Inglis made some remarks on the Conducting Power of Iodine.

Dr. Knox made some observations on Fluorine.

Mr. Black described a mode of detecting the strength of Spirits by diluting with Water.

Dr. Trail made a communication on the Aurora Borealis.

SECTION C.

Mr. Charlesworth read a notice of the Vertebrated Animals in the Crag-formation.

Mr. Bowman read an account of a visit to the Bone Caves of Cefn, in Denbighshire.

Mr. Ibbotson exhibited Geographical Models of Neufchatel, and of the Under Cliff in the Isle of Wight.

Dr. Daubeny stated the results of some experiments on the effects of Arsenic or Vegetables.

Professor Sedgwick and Mr. Murchison, communicated a paper "On the Classification of the old Slate Rocks, and true position of the Culm Deposits of Devonshire."

Mr. De la Beche read a paper on the Connexion of the Geological Phenomena with the Mines of Cornwall and Devon.

Professor Phillips made some observations on the removal of large Blocks or Boulders from the Rocks of Cumberland.

A communication was received from Dr. Riley and Mr. Stutchbury, on certain Saurian Bones discovered near Bristol.

Dr. Buckland produced a Bone, which had been found upon the red-sandstone in Bristol, supposed to be a remain of one of the rioters burnt at the Custom-house, the animal matter of which having been roasted out, the cavities became filled with lead.

The Marquis Spineto read a report of the attempts made to ascertain the Latitude of the ancient City of Memphis.

Dr. Buckland placed upon the table specimens of the engravings of some of the Fossils in the Bristol Institution, prepared under the direction of M. Agassiz; and also a Copy of the first volume of his Treatise on Geology, for the Bridgewater Treatises.

Mr. Fox read an important paper on the change in the Chemical character of Minerals induced by Galvanism.

Mr. Crosse made some communications of the highest interest on the formation of Artificial Crystals and Minerals.

Mr. Conybeare read a paper on the Coal-fields of South Wales.

Mr. Murchison communicated some remarks on the Geological relations of certain Calcareous Rocks, near Manchester; and also on the ancient Hydrography of the River Severn.

Lord Nugent read a communication respecting some Sea Rivulets in the Island of Cephalonia.

Mr. Charlesworth read a paper on some alleged fallacies in determining the ages of Tertiary Deposits.

Professor Forbes made a communication on the connexion of the Pyrenean Hot Springs with the Geology of the District.

The Rev. Mr. Clarke gave an account of some Hot Springs at Longleat.

SECTION D.

Dr. Richardson communicated, in several readings, his report on North American Zoology.

Mr. Rootsey announced the results of various experiments to extract Sugar, Spirit, &c., from Mangel Wurzel.

Professor Henslow made some observations on the formation of Sugar in Plants.

Mr. W. G. Hall read a paper on the acceleration of the Growth of Wheat.

Mr. Bowman read a paper on the Longevity of Yew Trees.

Mr. Ball gave an account of a new species of the Seal.

Dr. Hancock exibited a specimen of a new and scandent species of *Norantea.*

The Rev. Mr. Hope exhibited an Hermaphrodite Lucanus.

Mr. Hope read an interesting paper on certain Notions of Antiquity derived from the Ancients.

Mr. Hall introduced the subject of the application of Lime as a manure.

Colonel Sykes made some observations on the fruits of the Deccan, of which he produced drawings.

Mr. J. T. Mackay read a report on the Geographical distribution of Plants in Ireland and the West of Scotland.

Mr. Royle introduced the subject of Caoutchouc, with some interesting particular's of its Importation and Application in Manufactures.

Mr. Duncan brought forward the subject of the Luminosity of the Sea, for the purpose of eliciting information respecting so beautiful a phenomenon.

Dr. Hancock gave an account of the Cow-Fish, or River-Cow.

Dr. Macartney read a paper on the mode of preserving Animal and Vegetable Substances.

The Rev. Mr. Hope read a communication from Mr. Ruddon, on the means of obtaining Insects from Turpentine.

Mr. Carpenter read a communication on the "Criteria of Species," founded on the views of Dr. Prichard.

SECTION E.

Dr. O'Beirne presented a Report from the Dublin Committee on the Pathology of the Nervous System.

Dr. Prichard read a paper on the Treatment of Diseases of the Brain.

Dr. Houston described a twin fœtus, born without brain, heart, or lungs.

Mr. Carmichael read a paper on Tubercles.

The London and Dublin Committees forwarded reports on the Motion and Sounds of the Heart.

Mr. Greeves introduced a paper on the Gyration of the Heart.

Dr. Brewster read a paper on the Polarization of Light.

Dr. Canon read a paper on Absorption.

Dr. Hodgkin communicated some observations on the connexion between the Veins and Absorbents.

Dr. Reid produced a short exposition of the Functions of the Nervous Structure.

Dr. Macartney exhibited a portable probang, and read two short papers, one on the Organs of Voice in the New Holland Ostrich; and the other on the Structure of the Teeth.

Mr. Walker read a paper on the Nerves and Muscles of the Eyeball.

Mr. Adams made some observations on the Pathological condition of the Bones in Chronic Rheumatism.

Mr. Hettling explained a New Mode of removing Ligatures.

Dr. Evanson made a report on a fracture of the neck of the Thigh-bone.

Mr. W. B. Carpenter read a paper on the Origin of Parasitic Animals.

Dr. R. T. Thompson explained the Chemistry of the Digestive Organs.

SECTION F.

Dr. M'Cleland communicated, at great length, some statistical facts connected with the former and present state of Glasgow.

Mr. Kingsley presented various tables relative to the Revenue and Exenditure of the United Kingdom.

Baron Dupin made some observations on a paper which he produced, ntitled, "Researches relative to the Price of Grain, and its influence on the French Population."

Mr. Porter produced a report of the Effects of Vaccination.

Colonel Sykes, from the Royal Asiatic Society of Great Britain, announced the formation of a Committee of that Society, for the purpose of collecting Statistical information respecting India, chiefly with a view to the romotion of commercial intercourse.

Mr. Gregg's Outlines of a Memoir on Statistical Desiderata was read.

Dr. Lardner made an important communication on the Effects of Railoads on International Communication.

Mr. Taylor, Treasurer of the Association, communicated a paper on the Mineral Riches of Great Britain.

Dr. Yellowly read a paper on the employment of Spade-husbandry.

Professor Forbes detailed the results of experiments on the Height, Weight, and Strength of 800 individuals, natives of England, Scotland, Ireand, and Belgium.

Dr. Collins communicated a paper on the Periodicity of Births.

Baron Dupin exhibited two Maps of Great Britain and Ireland, curiously haded to show the comparative Density of Population, and the comparative tate of Crime.

A Report from the Manchester Statistical Society, on the state of Educaion in Liverpool, was read; also, the Statistics of popular Education in Bristol.

Lord Sandon moved a recommendation to the East India Company to ake measures for procuring a census of the population under their government.

SECTION G.

Professor Moseley made some observations on the Theory of Locomotive Carriages.

Mr. Russell, of Edinburgh, laid before the Section the result of certain experiments on the traction of boats in canals at different velocities.

Mr. Henwood communicated a paper on Naval Architecture.

Mr. Corsham described certain improvements in Neper's Rods for facilitating the multiplication of high numbers.

Dr. Daubeny explained an instrument of his contrivance for taking up Sea Water from any given depth.

Mr. Braham explained certain improvements made by him in the Mariner's Compass.

Mr. Price exhibited a model of a new construction of Paddle-wheels.

Mr. Chatfield read an Essay on Naval Architecture.

Mr. Enys gave an account of the working of the Cornish Steam-engines.

Mr. Pinkus read a paper on the transmission of power by the rarefaction of air.

Dr. Lardner delivered, at different sittings, some highly interesting discourses on Steam Communication with various foreign parts.

The communications made by Messrs. Lubbock and Whewell, in he section *of Mathematics and Physics*, on the subject of the Tides, were *f peculiar interest and importance.*

There appeared, from a comparison of the observations of Mr. Hutchinson, of Liverpool, and Messrs. Jones and Russel, in London, with the tide-theory of Bernouilli, an extraordinary agreement between observation and theory, so far as the height of the tide is concerned; and an apparent disagreement in respect to the time of the tide. The tide, as our readers are aware, is dependent principally upon the moon, and partially upon the sun; the effects of the attractions of these bodies being sometimes superposed, and sometimes opposed, and passing within a lunar month twice through every intermediate stage between superposition and opposition. Upon the declinations of the sun and moon, upon their right ascensions, and their parallaxes, elements determining their position in respect to the earth, depend then the height of the tide; and the height calculated by theory from these elements, agrees, it appears (from a course of observations continued through 19 years in London, and a longer period, we believe, at Liverpool), so completely with the real height of the tide, that, according to Mr. Lubbock's observation, observation may be considered as, in this respect, an absolute verification of theory, the difference being less than might be supposed to occur in making and registering the observation.

Such was the *agreement* of the theory and observation. The anomaly was this: The effect of any relative position of the sun, moon, and earth upon the tide, at any place, might be supposed to be produced at the instant when they were in that position. Now this is not, it appears, the case.

That effect is not in reality produced until five lunar half-days afterwards; or, to speak more correctly, the true effect of any relative position of the sun, moon, and earth, is undoubtedly produced at the precise moment when they are in that position; but that effect does not develop itself under the form given by Bernouilli's theory until five lunar half-days afterwards. Thus, then, it appears that the statical theory of Bernouilli is wrong as to certain of its elements, but that the operation of the causes, whence the error of the theory results, is, after the expiration of five lunar half-days, neutralized.

Another most important and unexpected fact, stated by Mr. Lubbock, was that the height of the tide in any place was certainly and sensibly affected by the height of the barometer. The tide is higher as the height of the barometer is less. M. Daussy (who was, we believe, the first to observe this) found that a fall of the barometer 0.622 parts of an inch, caused the height of the tide in the port of Brest to be increased by 8.78 inches. And Mr. Dessiou has found, by a careful comparison of the tide-observations made in the port of Liverpool, in the year 1784, by Mr. Hutchinson, that the height of the tide was there affected by the height of the barometer; so that, *cæteris paribus*, it rose one inch on an average for each tenth of an inch of the fall of the barometer.

Mr. Lubbock was followed by Mr. Whewell, who stated that the observations of Mr. Bent on the tides of the port of Bristol did not exactly agree with the results of Mr. Lubbock, a fact which might probably be explained by the local causes, whose operation is there so remarkable, that at Bristol the tides rise 50 feet, whilst lower down the channel they rise only 20 feet. The discrepancy being of this kind, that dividing the elements of the theoretical tide into two,—those which depend upon declination, and those which depend upon parallax,—the time by which the observed follows the theoretical effect is different in the one case from what it is in the other.

The more valuable part of Mr. Whewell's communication had, however, reference to a proposition for determining permanently the relative level of land and water. "If," said he, "two horizontal lines could be drawn, and permanently marked on the earth's surface, at right angles to one another; a line, for instance, from Bristol to Ilfracomb, and from this, another across the country towards Lyme Regis, then observations on the height of high and low water, made through a long period of time, and referred to different points of this line would show whether that portion of the island on which these lines were traced, had, or had not, altered its position in respect to the level of the sea; whether that alteration of position were one whose direction was east and west, or north and south, or towards any intermediate point of the compass. And thus might be set at rest a question which has long occupied the attentions of geologists, and which has acquired of late peculiar interest, from what has been said of the alteration of the level of Sweden. It is earnestly to be desired that this valuable suggestion should be acted upon.

Mr. Jerrard, a gentleman resident, we believe, in Bristol, but well known in connexion with the University of Cambridge, had at the previous meeting of the Association submitted to the Section of Mathematics a theorem, the very designation of which had produced an extraordinary sensation among its members. It was a theorem, the object of which was no other than "to solve, by a general method, equations of the fourth degree." Our readers are aware that a simple equation, one of the first degree, is one of very easy solution, that an equation of the second degree, commonly called a quadratic, presents greater difficulties; these, however, are overcome by every school-boy; that an equation of the third degree or a cubic, although it resisted long the efforts of the earlier mathematicians, was yet long ago solved by that philosopher, astrologer, physician, and madman, Cardan, who believed the solution to have been communicated to him by a familiar spirit. Descartes at length solved an equation of four dimensions, and a second solution was effected by Waring; and here analysis was arrested; no method was known for the solution of equations above the fourth degree: the method of Mr. Jerrard was one which proposed to itself the solution of equations of every degree. It was one of great complication and labour, lying on the very verge of analytical knowledge, and requiring, previous to any investigation which might verify it, an abundance of labour, and the first order of mathematical power; it was accordingly referred to Sir William Hamilton, to report upon it at this meeting of the Association; and 80*l.* were placed at his disposal, to be paid to the person whom he might employ to perform the actual calculations of the roots of certain equations, according to this method.

Sir William Hamilton reported, in the first place, that he had not employed any portion of this money; that he had otherwise satisfied himself of the merits of Mr. Jerrard's method, which he discussed at considerable length, and greatly eulogized; but which did not effect the object which he had proposed. The following account of Mr. Jerrard's method is that given by Mr. Peacock, of Trinity College, Cambridge; we copy it as most correctly reported in the *Bristol Mirror*.

Mr. Peacock expressed his concurrence with most of the observations of Sir William Hamilton and Professor Babbage, and observed that the importance and value of Mr. Jerrard's researches ought not to be altogether estimated by his success in the general solution of equations of higher degrees than the fourth. By means of more correct definitions of symmetric functions than those which had been commonly employed, and by the separation of the symbols of operation from those of quantity in the manner first proposed by Arbogast, and by the adoption of a conventional notation equally simple and comprehensive, Mr. Jerrard had succeeded in the expression of symmetrical products of any entire rational functions by formulæ almost equally simple with those of the binomial and multinomial theorem. He was thus enabled to effect the transformation of equations, when the new unknown quantity expresses any rational and entire function of that in the original equation, the co-efficients of the new equation being all included in his general formula. Mr. Peacock considered this method of Mr. Jerrard as constituting a very important addition to the power which has hitherto been possessed by analysts in the treatment of symmetrical functions, and in the transformation of equations,—Ischirnhausen proposed to reduce equations of all orders to binomial equations by transformations which destroyed simultaneously all the intermediate terms; the application of this method requires the elimination of a series of unknown quantities from equation (whose terms are expressed by Mr. J.'s general formulæ) of the first, second, third, and regularly-ascending degrees: the final equation which results (for equation beyond the third degree) rises to a higher degree than that of the equation to be solved. Mr. Jerrard, however, by the introduction of a sufficient number of terms into his reducing equation (whose co-efficients are to be determined by the required conditions,) has shown in what manner the second, third, and fourth terms, or the second, third, and fourth terms of any equation beyond the fourth degree may be simultaneously exterminated, by means of equations of the third or fifth degrees. Mr. Peacock observed that he considered the process which had been employed by Mr. Jerrard for this purpose as eminently ingenious and original, and that he was unable to discover any defect in his reasoning, though the possibility of solving this very considerable problem had escaped the observation of Lagrange. Mr. P. said the extension of Mr. Jerrard's process to the simultaneous extermination of four successive terms of an equation of five dimensions, or its reduction to one of De Moivre's solvible forms, would require the introduction of more terms into the reducing than into the original equation, and would consequently introduce relations amongst its co-efficients, which were necessarily furnished by the original equation, and not by the conditions which the process of solution require to be satisfied. It was for this reason that Mr. Peacock was disposed to doubt the applicability of Mr. Jerrard's method to the solution of equations beyond the fourth degree. Mr. Peacock proceeded to observe that every method which had hitherto been proposed for the general solution of equations had been shown to be impracticable, and that he was himself so far influenced by Abel's reasoning as to believe that the general expression for the root of an equation of the fifth degree (if any such existed) would be an unknown transcendent. In inquiries of this very difficult nature, which approach the extreme limit of the abstract speculations of the human mind, it would be equally unsafe and unphilosophical to assert that the solution of this or any other problem was impossible, unless it is distinctly defined in what sense that impossibility is understood; the researches of analysts are perpetually leading to a knowledge of the existence and properties of new transcendents, the necessary results of symbolical language and reasoning, whose complete developement and expression, by the ordinary operations and signs of algebra, is altogether impracticable; and it is to expressions of this kind that we must probably look in the further prosecution of such inquiries as those which were the primary objects of Mr. Jerrard's most valuable and original memoir.

The report read by Professor Phillips on the Interior Temperature of the Earth, was in so far interesting as it tended to show, from experiments made by Professor Forbes in the mines of the Lead-hills, by Mr. Buddle, at Newcastle, and Mr. Anderson, at Wearmouth, that the received theory of the increase of temperature as we descend, is correct. In one of the examples cited, the thermometer stood at 78° in the mine, and the mean temperature at the surface was 47½°, whilst the depth was 525 yards. In a mine at Newcastle, at 280 yards below the surface, with a mean temperature of 47½°, the thermometer stood at 64°, being very nearly the augmentation of a degree for every fifteen yards, which proposition might be taken as the average in a determinate ratio. In a mine at Manchester, at a depth of 337 yards, it was 62°; in a salt-mine at Northwich, 112 feet deep, 52°; and in a mine at Bedminster, near Bristol, which had been ascertained within a few days, at a depth of 170 yards, it was 64°. In a pit at Newcastle, 538 yards deep, or about three-tenths of a mile, where the temperature above averaged 47½°, that below was 72°, and in two other observations, 74½° and 78°. So well were these principles known at Paris, that he had been informed a well was in process of being sunk for the purpose of obtaining hot water, which he had no doubt did exist in the interior.

Among the most interesting of the communications made to the meeting, were those of Mr. Russel, of Edinburgh, on the subject of Traction on Canals; and as connected with this subject, that of a certain wave which is, it appears, produced in a canal when a body is put in motion, analogous to that which might be produced if we suppose a mass of water suddenly to be let in at the extremity of the canal. This wave, Mr. Russel stated, to move along the canal with an *uniform* velocity, between which and the depth of the canal there was this remarkable relation, that the velocity was exactly that which a heavy body would acquire in falling *freely* through a space equal to one half the depth of the canal. Proceeding continually with this same velocity, but diminishing continually in height, the same wave had been followed by Mr. Russel, and distinctly seen for a mile and a-half; and it had been perceived in another case to exist at a distance of three miles from the place where it originated. Since the velocity of the wave depends upon the depth of the canal, it will be affected by any occasional shallowness, and will be less near the sides than at the centre; and, moreover, canals differing from one another in depth, will have different corresponding velocities of the waves which are propagated through them. Now *upon this depends a most* important condition of the traction of *barges upon the canal at high* velocities.

The received law of the resistance of fluids, is one by which it is made to vary as the square of the velocity; so that a boat drawn through a fluid with twice the velocity of another, would require four times the traction; drawn with three times the velocity, it would require nine times the traction, and so on. And if this law had no modification in practice, to draw a boat at nine miles an hour would require nine times the traction, (nine times the actual pull of the horse, to say nothing of the rate at which he must go,) that it would to draw it at the ordinary rate of three miles an hour. Thus, the possibility of impelling canal-boats to any useful purpose with any but very low velocities was never thought of. Boats are, nevertheless, now drawn commonly on the Forth and Clyde canal, at the rate of nine or ten, or, we believe, in some cases, at twelve miles an hour, and with less traction than they can be drawn at four, five, or six miles an hour. The wave observed by Mr. Russel, and the uniformity of its motion, connects itself with this fact. The boat, when put in motion, sends its wave before it, whose motion has nothing whatever to do with that of the boat, but depends only upon the depth of the canal. In the Forth and Clyde canal, its velocity is eight miles an hour. Whilst the boat moves then at less than this velocity, it is behind the wave; and so long the resistance is continually greater as the velocity is greater, and according to the received law of the squares.

At seven miles and a half, or eight miles and a half an hour, the boat overtakes the wave, and is lifted upon it, and then the traction, instead of being less than it was at seven or seven miles and a half, became suddenly greater. It was in one instance observed to be at 7½ miles an hour, 330 lbs. by the dynamometer, while at 8½ miles, instead of further increasing, it fell to 236 lbs. The complete explanation of this fact remains, perhaps, to be given; the fact itself is unquestionable. Its application to the navigation of canals is evident.

Every canal has, according to its depth, a certain velocity of wave, on which depends the rate at which a fast boat may best be impelled.

This communication of Mr. Russel was made to the Section of Mechanical Science, which had been opened by a discussion between Professor Moseley and Dr. Lardner on an important point in the theory of rail-roads. It appears, that in certain calculations made to determine the expense of working a given load on two differerent and rival lines of rail-road, an assumption had been made, that provided the inclination did not exceed the angle of repose, the additional expense of coals in working the load up the incline would be compensated by the diminished traction down it. This position Professor Moseley denied. It was, he admitted, true in respect to the direct traction arising from gravity, and in respect to the traction arising from the friction of the train on the incline; but it was not true in respect to the friction of the machinery of the locomotive-carriage itself, which friction he stated to be a considerable element of the whole resistance, amounting always to one-fourth or one-fifth of the whole traction. This friction of the machinery, he stated to be composed of two parts: 1st. The passive friction of the engine being that which would be necessary to put the wheels in motion, if the carriage was lifted from the ground. 2ndly. The friction of the engine, resulting from the force which it overcomes, and therefore proportional to the traction.

The first of these resistances is, in passing over an incline, worked over the two sides of the incline, instead of over the base; and the second is increased enormously in the ascent of the plane by the traction, whilst in the descent it is only diminished until it is equal to that on the horizontal plane; leaving on the whole a very considerable loss of power, and therefore of fuel, by the ascent. In the course of this discussion, Dr. Lardner stated that he had entirely arrested the descent of a train upon an incline by scattering sand upon it, and he suggested that great practical advantage might be derived from attaching watering-pots to the carriages, by which the rails might be watered before the wheels, and the friction materially reduced.

An extremely interesting and important paper was read in Section A, by Professor Powell, on Refractive Indices.

The determination of the refractive indices for definite rays of the solar spectrum, marked by the dark lines, from the direct observation of their deviations produced by prisms of different substances, first proposed and executed by Fraunhofer, for ten media, solid and fluid, was carried on by Mr. Rudberg for ten more cases. The necessity of an extended series of such determinations was pointed out by Sir J. Herschel and Sir D. Brewster, and was further urged by a special recommendation of the British Association. Mr. Powell, by a simple and most ingenious apparatus, has ascertained the refractive indices belonging to each of the standard primary rays for various media, which may be considered as a most valuable contribution to this branch of science.

The following are among the most interesting communications in CHEMISTRY and MINERALOGY:—

Dr. Hare (of Philadelphia) described the mode he had devised for Blasting Rocks in America; the apparatus consists of two copper-wires joined by a small platina-wire, in the middle of a tin case containing the gunpowder to be exploded. The two extremities are attached to a calorimeter, and when this is set in action the voltaic ignition is effected. The advantages of this mode are, that the action is instantaneous, and may be carried to a considerable distance from the persons employed. Dr. H. stated that 12 simultaneous discharges in a rock had been made at 150 feet distance; and he had no doubt that very large blocks for columns, &c., might thus readily be detached. These combined explosions may, of course, be made beneath water, and afford an obvious mode of blowing up rocks impeding navigation.

Besides these advantages, the causes of human suffering and calamity are lessened. If the explosion is not effected the instant the voltaic apparatus is in full use, then the cause of failure may fearlessly be sought after the voltaic apparatus has been disconnected, and thus the many accidents resulting from approaching and disturbing a delayed but ignited train are avoided.

Instances were given of this description, and a painful case related, where the ignited train had not exploded; and after two hours had elapsed, the unfortunate man descended into the mine, which was filled with suffocating gases, evolved from the smouldering combustion of the train and some tallow, &c., accidentally present; he was followed, at different intervals, by his two sons, who also fell victims. By Dr. Hare's method of firing the gunpowder, these and other obvious dangers are avoided.

Dr. Charles Henry read a very interesting paper upon the Power of certain Gases to interfere or prevent the union of Oxygen and Hydrogen by the action of Platina.

Dr. H. described the peculiar powers of platina to unite oxygen and hydrogen gases slowly when the metal was solid, or in the state of foil or leaf, as in Faraday's experiments. He referred to the well-known power of the spongy platina, which quickly determines the union of the gases, as shown by Dobereiner's original experiment; and to the rapid effects accompanied by incandescence when the fine black platina-powder prepared by Liebig's process is employed. Carbonic oxide, olefiant, and other gases, either prevent an explosive mixture from combining, or retard the action, and they do so, Dr. H. found, because the platina exerts the power of uniting the carbonic oxide to the oxygen to produce carbonic acid. He states that all the interfering gases unite with oxygen, and they are determined by the platina in the order of combustion. He pointed out the influence of temperature in exalting the chemical energies; and also that the interfering gases prevented explosive mixtures from being united by the electric spark.

Dr. Turner considered the experiments and explanations of Dr. Henry as very satisfactory.

Mr. Herapath showed experiments for detecting very minute portions of Arsenic. The tests by ammonia-sulphate of copper, (Scheele's green,) and ammonia-nitrate of silver, were rendered more delicate by placing a drop of tested fluid on blotting-paper placed upon chalk, and so absorbing the solutions from the arsenical precipitates. $\frac{1}{10000}$ of a grain of arsenic might thus be rendered evident, and preserved for inspection.

He showed the recent decisive mode of discovering arsenious acid, and for experiment, he mixed the arsenical substance with embarrassing matter, such as fat, &c.

Mr. W. West, of Leeds, read a paper on a means of ascertaining the presence and proportion of substances diffused in small quantities through the atmosphere.

The author proposed, by means of self-adjusting wind-sails, to draw large measured quantities of atmospheric air through liquids calculated to combine with, and detain the foreign substances expected, and thus determine the presence and quantities, for example, of saline matters on the shores of the ocean, or when carried by gales over inland countries. In the neighbourhood of towns it is obvious that the combustion of coals, and the processes of art must contaminate the air. Indeed, Dr. Macculloch some years ago pointed out sulphate of ammonia, and other *salts* existing in the London air.

Dr. Thomson remarked, that in Glasgow and some English towns, silver could not be kept many weeks before it was attacked by sulphur vapours, while on the Continent he had seen plate which had been exposed for fifty years, not so much affected.

Professor Johnston brought before the Section his chemical tables, published in 4to., of which a specimen, entitled "CHEMICAL CONSTANTS," had been laid before the Association at their meeting in Dublin. The suggestion of collecting a series of tables of constants in nature and art was made by Mr. Babbage, and which he (Professor Johnston) had applied to chemistry. One important advantage likely to result from each table was, that by presenting to the mind all known facts, as it were in a single glance, they would enable it more readily and more distinctly to trace the mutual relations of *bodies, and more* easily to attain general and enlarged views of the leading *doctrines, objects,* and prospects of chemical science. But another advantage, and one which came more clearly within the scope and objects of the British Association was, that by collecting into one mass the scattered fragments of knowledge, some waste of time and labour might be spared to the assiduous experimenter; while by showing what, and how much really remained to be done, they would point out new paths on which he might enter, with honour to himself and advantage to the science. They might be expected also to enlist new labourers in the cause of original research, since there was no experimenter whose means were so limited, as not to be able to fill up some of the many blanks which those tables presented. These tables of Mr. Johnston may be expected to afford a new and exceedingly valuable mode of collecting and recording those facts and details now in MS., and which but for some such means would be kept from the world. Every experimentalist, ascertains facts, and tries numbers of experiments, the details of which are too minute for publication or recollection, if published separately,—and it is quite obvious that facts may be tabulated which otherwise would not be deemed worthy of separate publication,—we do most sincerely hope that each chemist capable of filling up a *blank*, would take the opportunity, at some future meeting of the Association, to give his information to Mr. Johnston.

Dr. Thomson read a detailed account of the experiments on the combination of Sulphuric Acid and Water he had made with great care, and distributed some tabular results of the alteration of the specific heat of the different mixtures of oil of vitriol combined with from one to ten atoms of water. He stated that his experiments, if repeated by others, must be made with the full knowledge that pure sulphuric acid is not made in Great Britain; all the acid of commerce contains nitric acid, sulphurous acid, (and, we may add, frequently muriatic acid also.) He asserted that sulphuric acid manufactured in this country would give results differing entirely from his. Any chemist, on diluting the oil of vitriol with water, will obtain acid vapours of these substances.

The specific gravity of pure oil of vitriol—(1 atom of dry sulphuric acid +1 atom of water) is 1.8422 at 59° of Fahr.

The acid Dr. Thomson used, was the acid obtained from Nordhausen, in Saxony.

Mr. W. C. Jones read a paper on a peculiar modification of Gluten, and on a peculiar volatile fluid. He stated that the compounds he described were procured in an elaborate analysis of wheat. The result of the quantity of its relative ingredients was very different to that obtained by preceding chemists, and from the care taken in the experiments, appeared to meet the approbation of the members of the section who were present. The gluten, of which he produced a specimen, differed entirely from that obtained in the usual way, being perfectly soluble in alcohol and water, and also possessed other interesting properties.

The peculiar volatile fluid was obtained from the distillation of the lignin of wheat with sulphuric acid, and appeared to be entirely unknown, opening, Mr. Jones observed, a wide field for investigation. He also proved that, contrary to all former analysis, the lignin of wheat contained nitrogen as one of its elements. He likewise showed that starch could be immediately converted into sugar by sulphuric acid, and which would appear to be in opposition to the experiments of Saussure. Mr. Jones stated that wheat from the South of Europe contained more gluten than English wheat, by two or three per cent.

Lithic Acid.—Mr. Herapath has observed that Lithiate of Ammonia is secreted *by certain insects;*—the Silk-worm Moth, the Privet and Puss *Moths, were among those from* which he had obtained it. The most curious point was, that it should be produced by creatures living entirely upon vegetable food.

Dr. Macartney read an interesting paper on the mode of preserving Animal and Vegetable Substances, in the course of which he stated, that by washing insects, skins of animals, or flowers, in essential oil of cloves, or indeed in any essential oil, they might be preserved for a great length of time without injury.

On the 18th of November last, Mr. Herapath had an opportunity of witnessing a brilliant display of the Aurora Borealis, which he thus describes.

"About nine o'clock on the evening in question, a *heavy* and *well-defined cloud* bounded the north-western horizon. It was surmounted on its upper curved surface by pale phosphoric light, which seemed to radiate from that surface, and occasionally, or in fact frequently, waves (or as they are usually called "streamers"), flitted from the principal light, proceeding to different distances, but in the same direction, and sometimes passing the zenith. These varied repeatedly in form, magnitude, and solidity, yet they were always so rare, that the stars (which were very bright at the commencement) were visible through their substance. It would have been impossible in the darkness (the aurora furnishing the only light) to form a judgment of the distance or the height of the pale light or of the streamers proceeding from it; but after keeping the eye fixed upon it for some time, I observed, that although the cloud had seemed immoveable, yet that small portions were being constantly detached from it; these were drifted away, and rapidly dissolved, none of them reaching the zenith, *and upon a large fragment having been blown off, it was surrounded by the same diffuse light as appeared on the great mass of cloud.* Having once seen this, I looked still closer after every fragment, and found that they all presented the same appearances. As the light declined, the detached portions were dissolved with more difficulty,—the air became hazy, and by half-past ten all was gone, the atmosphere being cloudy.

"It was evident, then, that the cause of the Aurora was something that attended upon water, as found in clouds, and that it was evolved during the solution of that water in air. The cloud itself was clearly electrical, being of the nimbus variety, *and in the midst of the streamers; I twice perceived a short, faint-blue electrical spark,* just as I have seen when imitating the Aurora in a partially-exhausted flask, when the electrical spark happened to be a little too strong. There is a very strong probability, therefore, that Aurora is merely *electricity passing off from a charged cloud in the act of dissolving in air that can take its water but not its electrical fluid,* which fluid while dispersing through a rare atmosphere, becomes evident to the eye."

Sugar from Flowers.—Professor Henslow referred to the formation of sugar in plants, and exhibited a crystal which had fallen from near the corolla of the common anodendron. Immediately after the saccharine matter had exuded it formed a crystal.

In the Section C of Geology, there was read a paper *On the old Slate-Rocks and Culm-Deposits of Devonshire*, by Professor Sedgwick, and J. R. Murchison, Esq., V.P.R.S., &c.

Mr. Murchison began by observing, that he was about to submit a mere outline of a more detailed memoir on the physical structure of Devonshire, *which, in conjunction* with Mr. Sedgwick, he purposed to lay before the *Geological Society* of London. One object they had in view was, to remedy the defects in existing geological maps, as to colouring subdivisions of formations, and another to ascertain by actual Sections the true position of successive deposits, and their natural subdivisions, so as to bring them into comparison with other corresponding deposits, and to determine their true place in the succession of British formations. By help of a Section the following succession of deposits in the ascending order was determined:—

1. A system of slaty rocks, containing a vast abundance of organic remains, generally in the form of casts; these rocks sometimes pass into a fine glossy clay-slate, with a true transverse cleavage,—sometimes into a hard quartzo-flagstone, not unusually of a reddish tinge, sometimes into a reddish sandstone, subordinate to which are bands of incoherent shale; in North Devon they are very seldom so calcareous as to be burnt for lime, but in South Devon, rocks of the same age appear to be much more calcareous. This series is finely exposed in the Valley of Rocks, and the Valley of the Lyn, but its base is nowhere visible in this line of section.

2. A series of rocks, characterized by great masses of hard thick-bedded red sandstone and red flagstone, subordinate to which are, bands of red, purple, and variegated shales; the red colour occasionally disappears, and the formation puts on the ordinary appearance of a coarse siliceous graywacke, subordinate to which are some bands of slate, but too imperfect to be used for roofing. This system contains very fine organic remains; it is several thousand feet in thickness, occupying the whole coast from the west end of the Valley of Rocks to Combmartin, being thrown back by a dip into the cliffs between Porlock bay and Linton; it re-appears in North Hill and the Quantock Hills.

3. The calcareous slates of Combmartin and Ilfracombe; of very great aggregate thickness, abounding in organic remains, and containing in a part of its range at least nine distinct ribs of limestone, burnt for use. This limestone is prolonged into Somersetshire, and is apparently the equivalent of the limestone on the flank of the Quantock Hills.

4. A formation of lead-coloured roofing-slate, of great thickness, and occupying a well-defined zone in North Devon, its upper bed alternating with, and gradually passing into, a great deposit of green, gray, and purple, or red sandstone, and sienaceous flagstone. These siliceous masses alternate with incoherent slates, and are in some places surmounted by great masses of red unctuous shale, which, when in a more solid form, generally exhibit a cleavage oblique to the stratification.

5. The Silurian System, resting conformably on the preceding, of great thickness, on the western coast of North Devon, occupying a zone several miles wide, and containing many subordinate beds and masses of limestone. In its range towards the eastern part of the county it gradually thins off, but its characters are well-preserved, and throughout it contains an incredible number of characteristic organic remains.

6. The carbonaceous system of Devonshire. This system is very greatly expanded, stretching in a direction east and west across the county, occupying the whole coast from the neighbourhood of Barnstaple to St. Gennis, in Cornwall, and on its southern boundary ranging so close to Dartmoor, that its lower beds have been tilted up and mineralized by the action of the granite. This great formation is therefore deposited in a trough, the northern border of which rests partly in a conformable position upon the Silurian system, and partly upon older rocks, partly of the division No. 4. Its southern border rests on the slate-rocks of Cornwall and Launceston, and on the north flank of Dartmoor. *From one side to the other* it exhibits an extraordinary succes*sion of violent contortions, but its* true place in the ascending section admits

of no doubt whatever. In some places it is overlaid by patches of green sand, and in one part of the coast, west of Bideford, it is overlaid by the conglomerates of the new red sandstone, which are seen for half a mile resting unconformably on its edge. The lowest portion of this vast deposit is generally thin-bedded, sometimes composed of sandstone and slate, with impressions of plants,—sometimes of indurated compact slate, both in an earthy and crystalline state. These beds are surmounted by alternations of shale and dark-coloured limestone with a few fossils; subordinate to these on the west side, are many thin veins and flakes of culm and anthracite.

On the eastern side of the county the coal is wanting, and the calcareous beds are much more expanded. On the south side of the great trough the calcareous bands and dark shales are well exhibited; but near Oakhampton are, as above stated, mineralized by the action of the granite. The higher beds of this deposit are well exhibited on the coast west of Bideford, and consist of innumerable alternations of ferruginous sandstone, flagstone, and shale, containing, in several places, concretions of ironstone, very often exhibiting impressions of plants; and one extended tract of country, containing at least three beds of culm or stone-coal, associated with shales, contains many plants of species not known in the true coal-measures. Though in a state of greater induration than the ordinary coal-measures of England, and in most parts almost destitute of any trace of coal, yet even in these respects it differs not from a great unproductive tract of the coalfield of Pembrokeshire. Therefore from the order of super-position, from the mineral structure, from the absence of that slaty cleavage which characterizes the older rocks, on which the deposit rests, and from the specific character of its organic remains, they classed it with the *carboniferous* series.

Of all the communications made to the Association at this Meeting, those which produced the greatest interest, and which are, probably, among the most important, were the two following.

The first was from Mr. Fox, a gentleman long known in connexion with Science, and largely connected, we believe, with the Mining districts of Cornwall.

Mr. Fox mentioned the fact, long known to miners, of metalliferous veins intersecting different rocks containing ore in some of these rocks, and being nearly barren or entirely so in others. This circumstance suggested the idea of some definite cause; and his experiments on the electrical magnetic condition of metalliferous veins, and also on the electric conditions of various ores to each other, seem to have supplied an answer, inasmuch as it was thus proved that electro-magnetism was in a state of great activity under the earth's surface, and that it was independent of mere local action between the plates of copper and the ore with which they were in contact, by the occasional substitution of plates of zinc for those of copper, producing no change in the direction of the voltaic currents. He also referred to other experiments, in which two different varieties of copper ore, with water taken from the same mine, as the only exciting fluid, produced considerable voltaic action. The various kinds of saline matter which he had detected in water taken from different mines, and also taken from parts of the same mine, seemed to indicate another probable source of electricity; for can it *now* be doubted, that rocks impregnated with or holding in their minute fissures different kinds of mineral waters, must be in different electrical conditions or relations to each other? A general conclusion is, that in these fissures metalliferous deposits will be determined according to their relative electrical conditions; and that the direction of those deposits must have been influenced by the direction of the magnetic meridian. Thus we find the metallic deposits in most parts of the world having a general tendency to the E. and W. or N.E. and S.W.

Mr. Fox added that it was a curious fact, that on submitting the muriate of tin in solution to voltaic action, to the negative pole of the battery, and another to the positive, a portion of the tin was determined like the copper, the former in a metallic state, and the latter in that of an oxide, showing a remarkable analogy to the relative position of tin and copper ore with respect to each other, as they are found in the mineral veins.

The second was from a gentleman of ancient family and independent fortune in the county of Somerset, who, although unconnected we believe with any public society, the author of no published work, or even printed paper, and unknown in what is called the scientific world, has yet for many years been the cultivator of experimental science, with an assiduity of which few instances can be found, perhaps, in the list of professional philosophers, and with that unobtrusive liberality, which is a sure pledge of the sincerity of scientific ardour and the genuine love of knowledge. Mr. Cross has an electric apparatus, which is, we are informed, one of the most splendid ever constructed; and after having for many years particularly devoted himself to this important branch of experimental knowledge, his labours have at length been crowned with the important discovery which is detailed below.

Mr. Cross stated that he came to Bristol to be a listener only, and with no idea he should be called upon to address a Section. He was no geologist, and but a little of a mineralogist; he had, however, devoted much of his time to electricity, and he had latterly been occupied in improvements in the voltaic power, by which he had succeeded in keeping it in full force for twelve months by water alone, rejecting acids entirely. Mr. C. then proceeded to state that he had obtained water from a finely-crystallized cave at Holway, and, by the action of the voltaic battery, had succeeded in producing from,that water, in the course of ten days, numerous rhomboidal crystals, resembling those of the cave; in order to ascertain if light had any influence in the process, he tried it again in a dark cellar, and produced similar crystals in six days, with one-fourth of the voltaic power. He had repeated the experiments a hundred times, and always with the same results. He was fully convinced that it was possible to make even diamonds, and that at no distant period every kind of mineral would be formed by the ingenuity of man. By a variation of his experiments he had obtained gray and blue carbonate of copper, phosphate of soda, and twenty or thirty other specimens. If any members of the Association would favour him with a visit at his house, they would be received with hospitality, though in a wild and savage region on the Quantock hills, and he should be proud to repeat his experiments in their presence. Mr. C. sat down amidst long-continued cheering.

Professor Sedgwick said he had discovered in Mr. Cross a friend who some years ago kindly conducted him over the Quantock hills on the way to Taunton. The residence of that gentleman was not, as he had described it, in a wild and savage region, but seated amidst the sublime and beautiful in nature. At that time he was engaged in carrying on the most gigantic experiments, attaching voltaic lines to the trees of the forest, and conducting through them streams of lightning as large as the mast of a 74 gun-ship, and even turning them through his house with the dexterity of an able charioteer. *Sincerely did he congratulate the* section on what they had heard and witnessed that morning. The operations of electrical phenomena, instances of which had been detailed to them, proved that the whole world, even darkness itself, was steeped in everlasting light,—the first-born of heaven. However Mr. Cross may have hitherto concealed himself, from this time forth he must stand before the world as public property.

Professor Phillips said, the wonderful discoveries of Mr. Cross and Mr. Fox would open a field of science in which ages might be employed in exploring and imitating the phenomena of nature.

A variety of interesting communications were made to the Association by Dr. Lardner, on the subject of Steam Communication with America and with India—both subjects, at the present moment, of great public interest, and the former especially, at Bristol, where a company has been formed for the express purpose of navigating steam vessels directly, and by a single voyage, between that port and New York; and who are at present building a vessel of 1200 tons for that purpose. This subject he introduced in the Section of Mechanics, in a speech of which the following correct report is given in the *Times:*—

The very circumstance of the present and pressing interest which was felt upon this subject of steam communication to distant parts of the world—the fact that already considerable investment of capital had been made in such speculations—these were circumstances which would somewhat embarrass them in arriving at a safe and certain conclusion, because it would be obvious that they would, more or less, engender in the minds of a considerable portion prejudices which would be liable to bias their judgment, unless they used a good deal of self-control, and brought with it the exercise of their own judgment. He would, therefore, beg of every one, and more especially of those who had a direct interest in the inquiry, to dismiss from their minds all previously-formed judgments about it, and more especially upon this question to be guarded against the conclusion of mere theory; for if there was one point in practice, of a commercial nature, which more than another required to be founded on experience, it was this one, of extending steam navigation to voyages of extraordinary length.

Dr. Lardner said he was aware that since the question had arisen in this city, it had been stated that his own opinion was adverse to it; that impression was totally wrong: but he did feel, that as steps had been taken to try this experiment, great caution should be used in the adoption of the means of carrying it into effect. Almost all depended on a first attempt, for a failure would much retard the ultimate consummation of their wishes. He believed those in the section who knew him would readily acquit him of being forward to question the power of steam. He tendered the most unqualified allegiance to the sovereignty of steam, but he tendered the allegiance of a free and thinking subject to a constitutional monarch. He did not bow before the power of steam as an abject slave, and if he found a failure in the administration of that power, he attributed it entirely to the ministers. It was necessary they should devise some means of determining the locomotive duty of coals. It was a question to which he had devoted a good deal of time, and the only method he had been able to devise had been to determine the consumption of fuel per hour. He had made extensive observations, and he considered you must place 15 lb. of coal per hour for every horse. Mr. Watt some time since established a series of experiments with the view of determining the relative consumption of fuel, and the result was this—that the consumption of fuel under the marine-boilers was one-third less than under the land-boilers.

A committee of the House of Commons, some time since, had to determine the expediency of opening a long steam-communication with India, and much evidence was given. In one case, the opinion was 8 lb.; in another, 9 lb.; and another, 11 lb. They would take nine months. And then came the question of speed. They were all well aware that there had been for some years in operation a line of steamers by Falmouth and Corfu; they touched at Gibraltar. On an average of fifty-one voyages, the rate at which they made their trips was noted, and the result was seven miles and a quarter per hour. They had, therefore, the conclusion, that the locomotive duty of 9 lb. of coals was seven miles and a quarter of distance. If, therefore, 9 lb. gave seven miles and a quarter in distance, one ton would give 1,900 miles for every horse power. Then they must look for average weather; the build of the vessel was such that they had not space to try more than 1¼ ton of coals for every horse power. Almost all the vessels with which the experiments had been made had the patent paddle-wheels, and they had been worked with the best coals.

The next question was, what modification the vessel must undergo when applied to steam-communication with the United States. In the Atlantic there were westerly winds which prevailed almost continually, and were extremely violent, and attended with a great swell of the sea; but it was an astronomical phenomenon which was very well understood. The outward voyage of the great packet-ships was generally estimated at forty days, the homeward voyage at twenty days, so that the entire voyage occupied sixty days. If, then, they assumed that the average of outward and homeward voyage to the United States corresponded with the average weather between Falmouth and Corfu, then they would arrive at this conclusion,—that the outward voyage was worse than the average in the proportion of four to three. If the locomotive duty of coals provided for the voyage between Falmouth and Corfu was 1900 miles for a ton per horse power, they must deduct from that thirty-three per cent.: in order to get what the duty would be on the outward voyage to New York, you must take a third from 1900, and you would have 1300 miles. By the direct line from Bristol to New York, the distance was 3500 miles; if you allowed one ton of coals for every 1300 miles per horse power, the vessel would require to carry two tons and one-third for every horse power in her engine: therefore this vessel must carry nearly three times the whole complement the Admiralty steamers could carry. Let them take a vessel of 1600 tons, provided with 400 horse power engines; they must take two and a third tons for each horse power, the vessel must have 1348 tons of coal, and to that add 400 tons, and the vessel must carry a burden of 1748 tons. He thought it would be a waste of time, to say much more to convince them of the inexpediency of attempting a direct voyage to New York, for in this case 2080 miles was the largest run a steamer could encounter; at the end of that distance she would require a relay of coals.

The question then became a geographical one, as to the best mode of accomplishing this. There were two ways which might be proposed; one to make the Azores an intermediate station, and to proceed from thence to New York; the other would be to proceed to some point in Newfoundland, and make that an intermediate station. The distance from Bristol to the Azores was 1300 miles, and from the Azores to New York 2400 miles, being twenty per cent. more than the steam-limit he had mentioned. There was a point called Sydney, in Cape Breton, where there were coal-mines worked to a profit by Messrs. Rundell and Bridge, but then that was 2300 miles; but if we *took our final departure from* some place upon the western coast of Ireland, *and there charged the vessel* with coals, the distance to Sydney would be only 1900 miles. The railroad system might be established in Ireland, which would be a benefit in more ways than one. London and all the southern section of the country would pour in their produce and population by the railway to Bristol. Dr. Lardner concluded, by counselling those who proposed to invest capital in this most interesting enterprise, to keep in mind certain points to which he would direct their attention. 1st. He would advise that the measured tonnage should correspond with the tonnage by displacement. 2d. To go to an increased expense in using the best coals. 3d. He would earnestly impress upon them the expediency of adopting the paddle-wheels shown in the section yesterday. 4th. He advised the proportion of one to four on the proper tonnage. 5th. He would impress upon them the expediency of giving more attention in the selection of engineers and stokers; it was a matter of the last importance, and a saving of thirty to forty per cent. With respect to the boilers, he would recommend copper ones, and, he advised that the coal-boxes should be tanked.

A second communication of great interest as it regards this subject was made by Dr Lardner, to the Section of Statistics, from which it appeared, that the intercommunication between places which have up to this time been connected by railroads, has, in every case which has been investigated, been increased in the proportion of four to one. Three cases, in which the data have been supplied, gave all of them this result. Before the completion of the railroad, twenty-six coaches plied between Manchester and Liverpool, and carried, on an average, 400 passengers daily; the railway has been in operation since 1828, and the average number of passengers carried every day by it, has been more than 1500. A second case was that of the railroad between Newcastle and Hexham; before its completion, the returns gave a communication of 1700 persons passing between these by coaches; the first ten months of the railroad gave 7060, being as before, in the ratio of four to one. The third case is that of the railroad from Dublin to Kingstown, which carries now an average of 3200 persons daily; whereas, before its establishment, there were carried between the two places an average of only 800 persons daily.

In the Medical Section, an interesting paper was read by Dr. Prichard, on Diseases of the Brain, detailing his new method of treating such disorders.

I am not disposed, said he, to believe that any material improvement can be made in the ordinary rules for the use of evacuants or measures of depletion, but I have no doubt that an important advantage may be gained by directing, in a particular manner, the mode of counter-irritation, and it is chiefly with the view of recommending this attempt that I have premised the foregoing remarks. Long experience has convinced me that the most efficacious way of applying counter-irritation in diseases of the brain, is a method not often practised in other places, which has been for many years in almost constant use at the Bristol Infirmary. An objection would probably arise in the minds of those who have not witnessed the application of this remedy, on account of its apparent severity. I hope to convince the medical section, and, through this opportunity, to make more general than would otherwise be done, the persuasion that the method of treatment to which I refer is by no means so painful or severe a remedy as it might be supposed to be, and that it greatly *exceeds in efficacy* all other means by which physicians have attempted to *relieve diseases of* the brain on a similar principle. The application I recommend is an issue produced either by means of a soft caustic, or, what is much better, by an incision over the scalp. The incision is most frequently made in the direction of the sagittal suture, from the summit of the forehead to the occiput. The scalp is divided down to the pericranium. The incision, when that method is used, or the aperture left by the slough, when caustic is employed, is kept open by the insertion of one or two, or, in some instances, three rows of peas. The discharge thus occasioned is considerable, and it obviously takes place from vessels which communicate very freely with the vessels of the encephalon. It would appear, *à priori*, very probable that an issue in this particular region, just over the sagittal suture, would have a greater effect on the state of the brain, than in any other situation, and the result of very numerous trials has abundantly established the fact. I can venture to assert, that in all those cases of a cerebral disease in which counter-irritation is at all an available remedy, an issue of the kind now described is, next to bleeding, by far the most important of all the means which have yet been, or are likely to be discovered. The kinds of cerebral disease in which counter-irritation is beneficial, include, according to my experience, all those complaints which are accompanied by usual stupor or diminished sensibility, excluding all affections attended by over-excitement, such as maniacal and hysterical diseases. In the latter, I believe such measures to be, for the most part, injurious.

A case has lately occurred in my practice at the Bristol Infirmary, which strongly exemplifies the efficacy of the treatment which I have recommended, and which I have fortunately an opportunity of bringing before the Medical section in the most convincing way. A youth, aged about eighteen, came into the Infirmary, labouring under complete amaurosis, which had been coming on gradually for a week or ten days before his admission. At that time it had become so complete that vision was entirely lost, and the pupils were totally insensible to light, even when the rays of the sun were suffered to fall immediately into the open eyes. At first he was freely and repeatedly bled from the arm and temporal artery, had leeches applied to the scalp, blisters to the nape of the neck, and took calomel so as to render his gums sore. Finding that no effect whatever was produced by these measures, I gave up the expectation which I had at first entertained of his recovering sight, but was resolved to give the remedies a complete trial. I ordered him to be bled *ad deliquium:* this took place after a small quantity of blood had flowed from his arm while he was in an erect posture. After a few days he was still perfectly dark; an incision was now made over the sagittal suture, from the forehead to the occiput: it was filled with peas; in three or four days, precisely at the time when suppuration began to take place, the patient declared that he perceived light, but was scarcely believed, since the pupils were still widely dilated, and quite insensible to a strong light. In the course of a few days it was quite evident that he saw: he could tell when two or three fingers were held up. For some weeks the iris was still quite inirritable, though vision had become in a great degree restored. The subsequent treatment of the case consisted chiefly in occasional leechings, purging, and low diet. When the issue healed, which was not till it had been kept open for some months, a seton in the neck was substituted: under this treatment the case has terminated in a complete recovery of the blessing of sight.

The following were among the subjects discussed in the Section D of Zoology and Botany:—

Dr. Richardson read the introductory portion of his report on North American Zoology, comprising remarks on the physical geography and climate of the country. *He noticed* three distinct mountain-chains, the principal one extending under the name of the Rocky Mountains, from the elevated plains of Mexico to the Arctic sea, rising within limits of perpetual snow and affording on its declivities inclined zones of equal temperature, running nearly north and south. He next mentioned the comparatively low range of the Alleghanas running near the Atlantic coast, and the more broken maritime Alps of California and New Caledonia, from which several lofty peaks arise. It was remarked that in these maritime Alps alone, of three ranges, recent or active volcanoes exist, which it was suggested might be one of the causes of the higher temperature of that coast. The great valley lying eastward of the Rocky Mountains, and extending from the Gulf of Mexico to the Arctic sea, and watered by the Mississippi, Missouri, Saskatchawan, and Mackenzie, was also noticed as exerting a manifest influence on the migrations of herbivorous quadrupeds, some tribes of birds and ansidromous fish. In the remarks on climate, the various ways in which the configuration of the land may contribute to influence the temperature of the atmosphere, were glanced at.

A paper by Dr. John Hancock, containing remarks on the Cow-fish, River Cow, *(Manatus fluvialis.)*

The animal was only now found in the lakes far away from the European settlements, and the name chosen for it was very inappropriate. Some authors asserted that the animal frequently weighed 8000 lbs., and measured twenty-eight feet in length, but he (Dr. Hancock) having seen many, and examined them, thought they very seldom exceeded 600 lbs. in weight, and 6 feet in length. The flesh of the animal is very good, very much resembling veal, very easy of digestion, and the soup made from it was delicious, and equal to turtle, though not so gelatinous; the flesh would also keep wholesome without salt for many months. The bones were highly esteemed by the natives, and when taken in a powder were very beneficial in complaints of the kidneys. It was also believed to bellow like a bull, and to fight desperately on some occasions. It moved through the water with great rapidity, not, however, by moving the tail laterally, as other fish, but horizontally, up and down. It had been asserted that this animal could not live on shore, but this he doubted, as it was unable to breathe like a fish, the respirative organs being nearly the same as those of terrestrial animals, and it was, therefore, obliged to come to the surface to respire, and always slept with its nose above water, under some shielding bank. Indeed, nature seemed to have placed it in an element which it was not fitted to; it was unable both to breathe and procure food under water, and it was thought that had it legs to walk on shore it would abide there. It was also suggested, that it would be desirable to find pasturage for these animals connected with small pools of water, and thus droves of the sea cow might be found; and a case was instanced of a sea cow being kept in a small lake in one of the West Indian Islands for 26 years, which became so tame as to be pleased with the human voice, to come when called, and to swim across the lake with children on its back, without plunging beneath the surface of the water. The upper part of the body approximated to the human form, and the posterior to the fish, and when it rose out of the water to gather food from the banks, it had much the appearance of what is called the mermaid, and from it, probably, the fables of mermaids and the tritons originated; particularly as the Indians usually had painted on the sterns of their canoes a figure similar to that which the cow fish presented when in the position described, which they styled "the man of the waters."

Professor Royle stated that in visiting the manufactory of the elastic web from **Caoutchouc**, or India-rubber, which is now applied to a variety of purposes, he was informed there was a difficulty in obtaining from South America a sufficient quantity of Caoutchouc or India-rubber for the purposes of the manufacture, and was therefore led to point out the variety of plants and countries from which the same substances might be obtained. A communication was first read from Mr. Sevier, the sculptor, who has made the principal discoveries in the properties of Caoutchouc and the commerce of Caoutchouc, by which it appeared that, since the removal of the duty, the importation of it had increased from 10 to 500 tons annually, and is soon expected to be 2000 or 3000 tons a-year, from its various uses as articles of dress, and ligatures of every kind, as well as for elastic ropes for the breaching of guns, and bands for driving machinery.

The earliest accounts, by Condamine, Aublet, and Priestley, were alluded to, and the South American tree, yielding Caoutchouc, was mentioned under the name *Siphonia elastica*, that of Penang as *Uruch elastica*, and the Indian as *Ficus elastica*, while other plants yield it in Madagascar, Mauritius, Singapore, and China. The natural families of plants to which all those yielding Caoutchouc belong, were stated to be *Cichorana*, *Lobeliaca*, *Apoegnea*, *Asclepiadea*, *Euphorbiacea*, and *Urticea*, among which are included *Arctocarpea*, all of which have milky juice, and are in considerable quantities in tropical countries; there could be but little doubt that many other plants of these families might be found to contain this useful substance as well as those which are already known to do so.

Besides these general results, it was observed, that many of the plants of this family were remarkable for the tenacity of their fibre, which fitted them for the purposes of rope-making, and that it was singular, that in the attempts to find substitutes for the mulberry-leaf in feeding the silk-worm, so many of the plants which they prefer, next to the mulberry-leaf, should belong to families which yield caoutchouc—as the lettuce-leaf, of the family of *Achnacea*, in England; the leaf of *Ficus religiosa*, the *Arctocarpea*, and the *Castor-Oil Plant* of the *Euphorbiacea*, in India. Considering that these facts were not likely to be accidental, the author was led to infer that something of the same kind must be contained in the juice of the mulberry, especially as it also belonged to the family of *Arctocarpea*, and having requested Mr. Sevier to make the experiment, the author was informed that he was perfectly correct in his indication, as the mulberry-juice also contained caoutchouc, whence it was inferred that the silk-worm requires some portion of this tenacious substance in its food to enable it to spin its silk, and the fact was communicated as probably of some practical value, as well as of scientific interest.

Mr. Hope subsequently remarked, that the dandelion, which had been previously noticed as yielding caoutchouc, was one of those employed as a substitute for feeding the silk-worm—a striking instance of the utility of men of different pursuits meeting and discussing subjects of this nature altogether.

To the Section F of Statistics, a very interesting communication was made by Baron Dupin, on the Influence of the Prices of Corn on Population. The baron prefaced his observations by stating, that it was formerly held to be indisputable that times of great plenty were favourable, and, on the contrary, that times of great scarcity were adverse to vitality. He had prepared returns in France, from the 86 departments—and in France the returns were officially and accurately taken—and also returns of baptisms, burials, and marriages, and he found that, in the 15 years preceding *the cholera, there* were more marriages and births, *and fewer deaths in the period* when corn was at a medium price.

In answer to questions put to him by different members of the Section, the Baron stated that,—

The difference in France between the lowest price and the medium price was as thirty-four to forty, and though that variation made but little difference to the consumer, it was not so to the grower, who had to estimate it on large quantities. In France the land was more divided than in England, the number of growers being no less than five millions; the ordinary tenure was one half of the produce, the leases being on three, six, and nine years; and hence it followed that, in times of scarcity, the proprietor bore one half of the suffering, as, when the prices fell, the contract still continued. He did not mean to contend for the position, that, abstractedly, a season of plenty was inimical to population, but the results he had given made it obvious that low prices were not, in themselves, the constituents of prosperity. The solution might be that, a sudden fall in prices, though regarded as advantageous to the mass of the population, might entail great suffering and distress on those engaged in agriculture, and that would account for the results he had given. Another fact, too, must be kept in view, that labour always bore a proportion in value to the price of commodities. At the close of the discussion the thanks of the meeting were unanimously voted to Baron Dupin for his interesting communication.

Colonel Sykes read a paper entitled, "On the utility of co-operating Committees of Trade and Agriculture, in the commercial and manufacturing towns of Great Britain; as projected by the Right Hon. Holt Mackenzie, and Mr. Forbes Royle, and advocated by the Right Hon. Sir Alexander Johnstone, and Sir Charles Forbes, Bart., for investigating more extensively the natural and artificial products of India."

The object of this paper was to incite the formation of committees in the manufacturing and commercial towns of Great Britain, either in co-operating with the Royal Asiatic Society, or independently, for the following objects: 1st. To ascertain what articles, the produce of India, now imported into England, are of inferior quality to those produced in other countries; to investigate the causes of their inferiority, and to explain and suggest the means of removing them. 2nd., To ascertain what articles, now in demand in England, or likely to be used, if furnished, but not yet generally forming part of our commerce with India, could be profitably provided in that country, or their place advantageously supplied by other things belonging to it; to take measures for making known in India the wants of England; and in England, the capabilities of India; and to suggest and facilitate such experiments as may be necessary to determine the practicability of rendering the resources of one country subservient to the exigences of the other. 3rd., To ascertain what useful articles are produced in countries possessing climates resembling those of the different parts of India, which are not known to this country, and, *vice versâ* to consider the means of transplanting the productions and transferring the processes of one country to another, and to encourage and facilitate all useful interchanges of that nature. 4th., With the above views, and for the sake of general knowledge and improvement, to consider how the statistics of Indian agriculture and arts, including climate, meteorology, geology, botany, and zoölogy, may be most conveniently and economically ascertained and recorded; and to encourage and facilitate all inquiries directed to these objects. Numerous illustrations of those views were quoted, particularly from Mr. Royle.

It was stated that, so recently as 1784, an American vessel arrived at

Liverpool with eight bags of cotton, which were seized, under the belief that America did not produce cotton; and now her produce is 400 millions of lbs. annually, the greater part of which is consumed by Great Britain; and it is a curious fact, that the native country of the sea-island cotton was supposed to be Persia. The Carolina rice, which sells at 5*d.* per lb., whilst East India rice sells at 2½*d.* and 3*d.*, originated in a single bag of East India, given by Mr. C. Dabors, of the India House, to an American trader. All the coffee of the West Indies originated in a single plant in the hot-houses of Amsterdam. It was stated that, in 1792, Mr. Browne, the resident at Cossimbazar, told the Council at Calcutta, that if he should think proper to send a few cwts. of lac to Europe, *it might* be procured at Calcutta. The annual consumption of lac in England is now estimated at 600,000lbs. A variety of other instances of a similar description, and of comparatively recent date, were adduced, to illustrate the importance of the subject now submitted to the consideration of the Section.

A very interesting paper was also read by Mr. Fripp, of Bristol, to this Section, on the statistics of the popular education in that city.

The population of Bristol and its suburbs (now incorporated in the new borough) according to the census of 1831, was 104,378, which number, at the usual rate of increase, 1½ per cent. per annum, must have become about 112,438 at the present time. The number of children attending schools, according to the returns obtained, is 14,717, of whom—

Attend Day School only	3609
,, Sunday *and* Day or National School . .	1645
,, Sunday Schools *only* , ...	9463
	14717

The total number of schools, of which returns have been obtained, is 128, of which—

		Boys.	Girls.	Total.
There are, Day Schools............	54 with	2209 ...	1921 ...	4130
,, Infant Schools	9 ...	609 ...	275 ...	1124
,, Sunday Schools.........	68 ...	5234 ...	5874 ...	11108
	128	8052	8170	16362

Deducting 1645 from the above total, *viz.*, 1526 in the day-schools, and 119 in the infant-schools, who attend also in the Sunday-schools, there remain 14,717, as before given, and the following are their ages:—

Under Five Years	1290
Between Five and Fifteen	12630
Above Fifteen	465
Not specified..	332
	14717

The proportion of the population between 5 and 15 being	24 per cent	or 26·985
The number of scholars, as returned between 5 and 15 is only	11·23 .	or 12·630
	12·77	or 14·355

Out of the 128 Schools—

Reading is taught in...............	128	Drawing is taught in	0
Writing	46	Mathematics	2
Arithmetic	39	Religion	126
Geography and History	15	Morals	119

The following table gives the number in connexion with the various *sects of religion:—*

	Schools.	Scholars.	Teachers.
Established Church	49	5680	214
Wesleyan Methodists	23	3899	626
Moravian, Independent, and other Dissenters	39	5171	591
No connexion with any particular denomination	17	1612	74
	128	16362	1505

Deduct duplicates:
Established Church 1305—Methodists 60
Baptists ...... 145—No denomination 135—— 1645
14717

The only important omission in the mass of facts thus collected, was with respect to the Roman Catholic Schools, no detailed returns of which, Mr. F. said, had reached him from the resident clergyman, though they had been twice applied for; he had, however, that morning obtained the total number of scholars attending those schools, which was 215—123 boys, 92 girls; being an increase of 58 boys and 33 girls since 1821. The returns of the children attending schools in connexion with the Wesleyan Methodists, included six Sunday schools (with 1260 scholars) which are not locally within the borough. but, being attended by many children resident in the outskirts of the city, and both supported and governed by a committee within the borough, they might with propriety be comprised in the borough-returns, whilst, on the other hand, it was probable that some charity-schools of minor consequence had not been returned. The returns were confined to public schools, whether day, infant, or Sunday, no account having been obtained of the different classes of private schools.

The only communication of interest and importance which remains to be noticed, or rather which at this late period we have time to notice, is one made to one of the evening-meetings, when Sir William Hamilton read a letter recently received from Sir John Herschel, at the Cape of Good Hope, giving an account of the progress of his observations on the nebulæ of that part of the southern hemisphere, a description of several of which he had forwarded to Professor Schumacher, for insertion in his Astronomical *Ephemeris.*

As an instance of the clearness of the sky, it was stated by an observer that in forty-two successive days there were only three in which he could not see Venus in broad day-light; and Sir John Herschel stated that he had also written a letter by the light of an eclipse of the moon. Under these circumstances, the starry heavens presented a brilliance of which the inhabitants of the northern hemisphere can have no conception; the line from Orion to Antinous being remarkably rich and brilliant, appearing as a continuous blaze of light, with, however, a few patches of the sky destitute of stars. The Magellanic clouds were described as curious objects, differing from other nebulæ apparently in the greater degree of condensation of the stars of which they were composed. He had also observed several planetary nebulæ, the appearance of some of which gave him at first the idea that they were real planetary bodies; and it was not until after he had observed one several times that he could divest himself of the idea that he had discovered a new planet more inclined than that of Pallas.

We must here break off. Although the account with which we have been enabled to furnish our readers will, we believe, be found clear and correct, as far as it goes, we acknowledge, with regret, that it has not been in our power to do full justice to the many excellent communications which were made on this occasion. Indeed, the interval between the close of the meeting and the period at which our Magazine must necessarily be in the press, is so very short, that we have, in several instances, been under the necessity of availing ourselves of some of the very able reports published in the Bristol and London papers, to complete our own notes of the proceedings.

Having, in the commencement of this Report, alluded to an accidental and very unimportant deviation from the programme of the meeting, in respect to that most useful auxiliary to the Association, the Inquiry-room, which, although announced as a place of reference on Saturday, on which day great numbers of persons arrived, was in reality a place where no information could be got until Monday; having, moreover, alluded to what we must be allowed to call the rapacity of the innkeepers, in advancing the price of their accommodations, a rapacity which has, as far as we know, no precedent, in respect to the meetings of this Association, it remains for us to bear testimony, as we do with great sincerity, to the cordial hospitality with which a large portion of the Members of the Association (those most eminent in science) were received and entertained in the private dwellings of the inhabitants; and to the munificient liberality which characterized the public reception of the Association. All the places of public resort, which could possess an interest with its members, were thrown open to them; and they were invited to visit all private collections of paintings, museums, manufactories, ancient buildings, &c. A steam-packet was provided to convey a geological party, on Friday, along the coast of Wales, round the Holms, up the coast of Somerset, and back to Bristol; and boats and a *déjeûné* were supplied for those who preferred to be rowed up the Avon, to the interesting excavations which are making to bring the Western Railway to Bristol. Refreshments were supplied in abundance for all who applied for them at the Theatre and at the Promenade; and, in short, everything was done on a scale worthy of the great and ancient British commercial city, which on this occasion entertained the British Association of Science*.

The Association terminated this, its sixth, assembly at midnight of Saturday the 27th of August, having sat the whole of the week. The number of members present exceeded that of any previous meeting, and amounted to about 1350. About 2000*l.* was voted for the prosecution of various inquiries, experiments, &c.

The seventh meeting is announced to be held in Liverpool, in the month of September, 1837. The day of meeting is yet undecided. The Presidents elected are, the Earl of Burlington, Dr. Dalton, Sir Philip Egerton, and the Rev. E. Stanley. Manchester, Newcastle-on-Tyne, Leeds, and Worcester, were competitors for the honour of receiving the Association.

* The sum of 800*l.* was, we believe, raised for this purpose, by local subscriptions.

MISCELLANIES.

Patent-Law Improvement-Bill.

Mr. Mackinnon has failed in getting this Bill even a second reading, and the Parliament has been prorogued. This "grievance" of the poor inventor must therefore still be borne. Mr. M. has given notice that he will make another attempt in the next session.

During the late meeting of the British Association at Bristol, Sir David Brewster, Mr. Babbage, and a few other members, feeling deeply the oppressive effects of the present law both on the inventive genius and the productive energy of the country, met several times, and passed resolutions expressive of the desirability of an early and an effectual removal of the evil. One of the parties stated that "he had been from his professional position, for many years a depositary of inventions, &c., which could not be patented under the present exorbitantly-high Patent-fees and stamps, but which would, whenever they were promulgated, put at least ten thousand hands to work on new engines, products, &c."

Practical Suggestions for a Reform of the Patent-Laws.

The following view of the subject was presented to the meeting of the British Association by Mr. James Simpson, Advocate, of Edinburgh. It may be observed, that this gentleman is one of the ablest supporters of the opinion, that the specification should be examined and reported upon by a commission of competent persons before the patent be granted.

"To influence a reform of the laws of patents for new inventions, was one of the leading objects of the formation of the British Association*. The subject, however, not having yet come under their consideration, I have humbly ventured, at the request of Sir David Brewster, and as having given some attention professionally to the subject, to offer a few observations upon it.

"The legislation which is demanded for the regulation of patents is essentially reformatory, and it seems the briefest way to obtain a notion of what it ought to be, to state the several evil consequences which follow the law as it exists. Generally, it may be said, that a patent, instead of being a reward for genius, and a privilege to be desired, is a trap for the unwary, a source of heart-breaking annoyance, and in many instances the cause of utter ruin: and for all this an enormous tax is imposed, almost entirely in the form of fees to high public functionaries who do nothing in the matter.

"1. The glaring inconsistency and absurdity of passing patents, *of course*, is the manifest root of all the evils attending the system. The patent is the privilege of a monopoly of the market of the patent article, as a reward for its *originality* and *utility*. But it is given to any one who asks it on his own affidavit, to his belief on these two important points. On both points the patentee, for he becomes such in the course of the official forms, on paying his fees, may be in error or in fraud; on both points, may have stolen another's invention, or taken a monoply of an article sold in every shop and market. If he has invaded the rights of prior inventors, or of the public, his patent is waste-paper, and ought to be so, and he must defend himself in a court of law. But the extreme looseness of the practice naturally gave rise to a leaning against the patentee, and the adoption of a number of rigid rules and technicalities, of all of which he was forced to get clear before he was secured of his privilege. There could be no distinction of persons, so that an Arkwright, a Watt, and a Dollond, was as much put at disadvantage as the least respectable patentee; and unable to overcome the vexatious obstacles that rendered their right a mockery, after expending vast sums, actually lost it on the most frivolous technicalities. Having, as I observed, had some practice as counsel in patent-cases, I can speak to the contemptible quibbles which it may cost the patentee thousands of pounds to clear away; and no patent can be held to be secure till it has passed the ordeal of a court of law, and cost from one to three thousand pounds*. I know that one patent which I had some share professionally in vindicating, and it is one of the few that has survived trials both in England and Scotland, has cost the patentees, the Messrs. Croxley, for it is the gas-meter, between £10,000 and £12,000. This oppressive result would be avoided, and its recurrence prevented by the previous investigation of claims for patents, by granting them *causa cognita;* and when the means are deliberately taken, to determine the points first of utility, and then of originality and priority, and to put the public at large on their guard, the right should be declared absolute, like that to any other property, and uncancellable during its term. All legal questions, with regard to the patentee, would then be limited to the question not of *patent, or no patent*, but *infringement*, or *no infringement*.

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* It is remarkable that from some cause or other, which ought to be ascertained and exposed, this "leading object," though repeatedly presented to the attention of the Managers of the Association, has never *been permitted to be* discussed. They *have uniformly denied it* admission to any *section. They did so this time at* Bristol.

"The diminution of litigation, and of course, of ruinous expense to the patentee, in his then position, must be obvious. The whole field of litigation in which he is now forced to prove his patent, would be closed in his favour, and infringement itself would be indirectly prevented, when all hope of defence is taken from the pirate on the insufficiency of the patent, and nothing remains but the question of infringement or not, the usual, and, very often, the successful, ground of attack.

"2. The next evil is the incompetency of common Juries, and even Judges, to try questions of patents in the short and peremptory sittings which can be allotted to them. The Counsel, who have the benefit of previous preparation, come of course prepared; but it is notorious that both Judges and Jury mistake the construction and operation of complicated machines, and it is their duty, not the counsel's, to decide. It is plain, that the previous investigation of the patent-right would relieve the tribunal that may be called upon to decide upon the question of infringement, of one great branch of the difficulty.

"3. A third evil amounts to a denial *in limine*, of the advantage of a patent to genius if accompanied by poverty. I allude to the enormous tax, in the form of official fees, laid upon patentees. I can see the justice and reasonableness of certain fees levied to remunerate the labour of a previous investigation of the patent-right, but why should the poor man, whose talent has discovered something that may benefit the country and the species, pay enormous sums to the Lord Chancellor, the Attorney-General, and other high functionaries? Why should these officers be paid by inventive genius, for no greater exertion than a nod of consent? But one-tenth of these fees would remunerate and establish the most perfect attainable system of investigation, and previous and subsequent inquiry.

"It is calculated that one hundred and fifty patents, on an average, pass in the year, costing about £45,000., of which about £7000. is paid to the Treasury, and £38,000. in form of fees to high officers, who do nothing whatever in the matter. It has been well termed the "*Patent-grievance*," and calls loudly for reform. It operates, in nineteen cases out of twenty, as a prohibition, and it is known to many who hear me, that there are hundreds of inventions which will never see the light, and may die with their authors, from the hopelessness of obtaining the necessary funds to pay the tax.

"The means of remedying these several evils are not now new propositions. They have been proposed by Wrottesley and Curwen, and given in evidence by the most experienced and enlightened witnesses, to Committees of Parliament. They have been discussed for years; their disadvantages as well as their advantages canvassed; and back we are forced to them as essential to the very existence of a rational patent-law.

"They are, First, That patents shall be granted, *causa cognita*, for original and useful inventions, and shall be vested rights like other property.

"Second, that the investigations previous to granting a patent shall be committed to Commissioners, consisting of eminent scientific men, and lawyers, of rank and character above all suspicion, and endowed by the government,—that endowment to be provided for by a moderate part of the tax now levied upon the inventor, and justly levied, because it is to pay for labour in his service; and, Thirdly, that official fees on patents be abolished, and a portion only retained to remunerate the Commissioners, who actually labour in the applicant's business.

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* By a very recent act passed on a bill introduced into the House of Lords by Lord Brougham, several of the most cruel disadvantages to which patentees were subjected have been removed; but the essential vice remains, or, as it may be called, the solecism that a patent-right signed by the sign manual, and passed under the great seal, is a right of no greater value than to be ruined *at law!*

"Some hold that patents should be obtained free, like the copyright of books. It would be desirable that this were practicable, but it is not. There is no doubting the originality of a book or the composition of its words and sentences, originality on the one hand and plagiarism on the other, are of easy establishment; but of the originality and utility of an invention in mechanics or chemistry, the investigation may be complicated and difficult. It will require labour, and that labour must be paid for, and by none other than the person who demands it. But he should have the labour at the lowest possible rate, to have it of the best quality, and that should be the sole expense of a patent. Suppose then that one hundred and fifty patents are passed in the year, costing the applicants 50*l.* each, for the three kingdoms and colonies, the sum of 7500*l.* would be raised to reimburse government for stated salaries, paid by it directly to the commissioners. It would be important that government should salary the commissioners, and take away temptation to pass frivolous inventions to increase the returns.

"In order to meet doubts of the trustworthiness of the commissioners, it has been proposed by many who have thought on the subject, that the commissioners should be divided into two, or even three, distinct and independent boards. If two, one sitting in London, and another in Edinburgh. If three, one in each of the three capitals; and that no patent should be granted that is not sanctioned by these independent boards. Local jealousies would then be met and remedied, and the state of the arts all over the empire better known.

"Into details this is not the place or time to enter. Each board would have its secretary and register, and the means of transmitting the results of their investigations to each other. That there might be a majority of boards, three would be better than two.

"Applicants would apply, as now, by petition to the king through the Secretary of State for the Home Department, who would at once send the case to one of the boards for investigation. Time and publication of the invention, by name and general description, would advertise the lieges whom it might concern. Competitors would be heard, their claims determined, admitted, or repelled, and the patent advised to be given or refused. The defeated party would then have a right of appeal to the other boards, and all the documents might be transmitted. The joint opinion, or that of a majority of the boards, to be final.

"No trial of the patent being afterwards compelled, it is only necessary to provide for questions of infringement. That I would humbly propose to render the function, the high function of the Commissioners, with appeal from one board to the others, and their verdict of infringement to be then pleaded in a court of law, to the end of obtaining damages. I shall beg to lodge with the secretary of this meeting the details, almost in clauses which might constitute a legislative measure, on the principles I have now laid down."

Physician, not necessarily an M.D.

Dr. Buckland, in one of the general meetings of the British Association at Bristol, asserted the natural right of the natural philosopher to the appellation of "physician." In speaking of a gentleman of high attainments in mathematics and physics, he boldly and happily styled him a physician. "Mark," says the Doctor, "not a medical one, from having obtained a diploma in the faculty of medicine, but a physician from an extensive acquaintance with the physics,—with the phenomena and laws of the material world." We hope this example of the excellent and sensible doctor will be steadily followed. In a conversation afterwards on the subject, it was suggested to prevent equivocation, that in the new and correct use of the word, it might be spelt "physicien," as the French have it, and that their division of the last two syllables might be adopted instead of the usual pronunciation "shun." We hope the College will be satisfied with this compromise.

Valuable Donation, by a Lady, to a Scientific Institution.

Irs. Page, of Speen, in Berkshire, the idow of the late Colonel Frederick Page, as recently presented to the Institution f Civil Engineers, of London, of which olonel Page was a member, "all such arts of his library and collections as elate to the objects of the profession, or a memorial of their late colleague."

As the Colonel, during a long life, ad been a zealous, intelligent, and iberal promoter and improver of Canal Navigation, and had carefully collected and preserved every fact and document of every canal in every country that came within his reach, the donation is of no ordinary value to such an institution. Besides several hundred volumes in English, French, German, Dutch, and Italian, and a large number of maps, plans, drawings, &c., the present contains the MS. engravings, drawings, and documents, which Colonel Page had been preparing for some years, and which he had intended for publication, under the title of *An Historical and Statistical View of Inland Navigation, and particularly that of England.*

Temperature of London in 1835.

From observations made regularly by Mr. Webster, at No. 43, Cornhill, London, the mean height of the Thermometer for the year 1835, at that place, was 30·836° Fahr. This is rather higher than the mean of the last fifteen years, which Mr. Webster states to be 29·562° Fahr.

New Fact in Electricity.

Professor Belli, of Milan, has published some experiments conducted by himself, which demonstrate that the negative electric fluid is dissipated in about half the time required by that which is positive. This fact had escaped the observation of Coulomb, and also of M. Biot. M. Peltier, greatly surprised at the difference thus found by the Italian Professor, is now engaged in an attempt to discover the cause.

Berlin University.

An official publication has issued at Berlin, which states that in the University of that city, there are at present 147 professors, 1677 matriculated students, and 470 persons who are studying for their pleasure.

Death of M. Ampère—Election of M. Savart.

The esteemed and venerable Ampère, a sketch of whose manner as a lecturer is given in Vol. I., p. 178, is lately dead. M. Ampère was Professor of Experimental Philosophy, *(la Physique Expérimentale,)* in the *Collège de France.* M. Savart, during the illness of M. Ampère, discharged his duties at the College. On his death, M. Savart was unanimously selected by the Professors as their candidate for the vacant chair, and he has since been elected to it by the *Académie des Sciences.*

Bronze Original of the Warwick Vase.

An antique vase of bronze, in remarkable preservation, and of great elegance, has been found in a garden at Cairo. It is said to be the one cast by Lysippus himself, the favourite sculptor of Alexander the Great, and to be the original from which the Warwick Vase has been copied. The French Consul, M. Mimaut, is reported to have refused some very large offers for its purchase.

Statistical Association of Normandy.

The Association of Normandy, instituted for the purpose of ascertaining the statistics of that part of France, have assembled successively in the chief towns of each district, to discuss and collect answers to a series of six hundred inquiries. These had been previously drawn up, and are intended to contain every point desirable to be known. The last meeting has recently taken place at Vire, at which 150 members of the Association were present.

Improved Composition for Crayons and Pencil Points.

Take equal quantities of resin and pitch, with as much shellac as is necessary for strength, and add to them fine pulverized black lead, of sufficient quantity, when melted, to form a soft paste; expose the mixture to a melting heat, and stir it with a trowel until it becomes soft and yielding. The composition may then be put into a heated iron mould, and forced through one or more holes, of any size required; it being then in a proper state for rolling, by which it acquires a polished surface. The rolls may then be laid in a straight position, cooled, gauged, and cut into the required lengths.—*Journal of the Franklin Institute.*

Gratuitous Institution for the Formation of Practical Naturalists.

The universal necessity for the establishment of a school for the education of the practical naturalist, has been repeatedly urged in *L'Echo du Monde savant*, a Parisian scientific periodical. The same work has recently announced, that the idea has been carried into execution. There ought to be as little doubt of its successful career, as there can be of the immense assistance it must furnish to the acquirement of an accurate knowledge of natural history in its most extensive sense. We subjoin some observations on the subject by M. Boubée, an eminent French geologist.

"Now that the study of nature is daily gaining so many proselytes to science, it cannot be matter of surprise that the number and contents of collections of natural history are rapidly increasing, and that the demand for persons skilled in the preparation of the objects of such collections, and experienced in their preservation, should become annually greater and more urgent. The scarcity of this class of practical naturalists is, in fact, everywhere so great, that besides several places in which the amateurs of natural history have the means of founding museums, but are deterred from so doing by the want of a naturalist who could take charge of a collection and direct the preparations, there are actually museums already commenced, which are in want of conservators and cannot obtain them. How many long-established institutions are there also, the officers of whose museums are now utterly inadequate to the duties which the mere extension of the catalogues imposes upon them! And are there not others, whose curators having neglected, or being incompetent, to keep up with the rapid march of their science, are so far in the rear, that the language used by their advanced associates is unintelligible to them?

"Under the eyes of the former, how many rare and precious objects are hourly perishing through the blunders or inattention of their guardians! Who can say what treasures are confined to the friendly damp and obscurity of cellars, that their testimony to the in*competency of the parties* to whom they *have been intrusted* may never obtrude *upon the public attention*? Under the care of the latter, how securely do old and exploded systems still hold their reign, and how strongly does the faded label, and disregard of all modern arrangement, which is evident in every case, testify the blissful ignorance in which their curator has quietly dozed for the last five-and-twenty years!

"Added to the want of practical naturalists, which the reforms in the old establishments, and the cravings of the new are producing, there are new associations organized for the acquisition, study, and preservation of local natural productions, arising in every department; expeditions for scientific purposes are fitting out, at great cost, for the exploration of the most distant countries; and the wealthy patrons of science are either visiting themselves, or commissioning others to visit and examine, at their expense, some of the numerous points of the globe on which Nature has deposited myriads of her wonders still unknown. In all these instances, experienced guides and intelligent assistants will be invaluable. But where are they to be found? And more, where could a lover of natural science prepare himself for such services by a course of actual research, conducted under the direction of competent teachers, and at a cost within the means of those who are not affluent?

"In no place that we know of does any opportunity of the kind exist. In no place, for instance, is Taxidermy practically taught; and public instruction imparted on the art of setting up the spoils of birds and quadrupeds, and giving to each specimen its characteristic expression and attitude. In no place are there lessons on the best modes of collecting and preparing the various classes of reptiles, of fishes, of insects, &c., and of preserving them, when prepared, from the numerous causes of destruction which surround them. Botany, it is true, may boast of her advantages; these have proceeded principally from the ease with which her subjects can be obtained and prepared, but even she has to deplore the losses and mischiefs which ignorance and inexperience are constantly producing. In Mineralogy and in Geology, the knowledge of the beds, judgment in the selection, and skill in the dressing, of the specimens, are possessed by very few individuals, whose position in society, and whose personal acquirements and tastes, are favourable to the communication of such knowledge to others.

"This extensive demand for assistants, and this dearth of instruction which is necessary to create them, has produced that scarcity of preparers, preservers, and travelling-naturalists, which is now severely felt in France, in Great Britain, in Belgium, and in all the northern states of Europe; in all places, in fact, where large public museums, or extensive private collections exist. Applications, in great numbers, are now perpetually addressed to the professors, &c., of the public establishments in Paris, for persons to whom the care of museums might be confided, and for naturalists who could explore skilfully, and report correctly, the phenomena of nature in new countries, or who would be competent and willing to associate themselves with expeditions of more or less magnitude entirely designed for scientific purposes, or with others of a commercial or political nature, but in which opportunities for scientific research and experiment would be afforded.

"It would, therefore, be rendering a most important service to science, if young men could be so prepared, by a short course of actual research and manipulation, as to become useful assistants to their leaders in science, or upright and intelligent guardians of the stores of natural riches already amassed. At the same time, it must also be evident that a new and honourable career would be opened to a numerous class of young men, who have an ardent thirst for knowledge of this kind, but whose friends are alarmed by the prevailing notion that scientific attainments are difficult to acquire, and that when acquired, they are not sufficiently remunerated to permit their possessor to live. In this state of public opinion, and particularly with regard to the latter notion, it is now desirable that every person who interests himself in the progress of the natural sciences should make it a duty to contradict, within his sphere of action, the assertion, that scientific studies cannot introduce young men to valuable connexions, nor open to them any profitable career. It is gratifying to be able to refute such mis-statements, and on safe grounds to assure all those who at present entertain such an opinion, that Science has *ceased to be so ungrateful to her cultivators. Let us take as an example, the branch of natural history* alone, let us point out the numerous sources of employment for its professors in France only. Natural History is now taught in all her universities and colleges, in numerous departmental institutions, in all the primary schools, and is about to be so in the secondary ones. The number of societies for its promotion, and of records for its history, are daily increasing, and so are the periodical publications, both those entirely devoted to the science, and others which, together with newspapers, devote a section. Botanical, horticultural, and zoological gardens and museums are common in the larger towns, and are even forming in the smaller ones. Enterprising individuals are continually undertaking new experiments and inquiries, associations are formed or forming in every part, the government is urgently demanding the completion of the geological maps of each department of France, and general commerce is becoming interested, on a grand scale, with the natural productions of all countries: hence are offered, to the rising generation particularly, numerous inducements, in a prudential point of view, to the study of natural history. The few who have hitherto devoted themselves to it, have generally been rewarded with offers of engagements from all quarters. And we repeat from our own knowledge, that there is in every part a great want of persons to fill situations, the duties of which would require an intimate knowledge of natural history; and we do not hesitate to avow, that even at Paris we have never yet obtained such a sufficient number of intelligent assistants in this class, as we ourselves desire to attach to our various publications.

"Impressed with these considerations, and anxious to supply this great deficiency of scientific labourers, the founders* of a museum for the collection of natural history on a large scale, in the Pyrénées, at St. Bertrand-de-Comminges, have resolved to adapt it to the practical instruction of naturalists and others who may be desirous of becoming conservators and scientific assistants in museums, and undertakers or associates in missions for the pursuit of natural history in every part of the globe. The operations and manipulations constantly carried on in the ateliers and laboratories attached to the museum, and the researches which are perpetually in train along the whole chain of the Pyrénées from the Atlantic to the Mediterranean, will necessarily introduce the students to every variety of object in the sea and on its shores, in regions of the lowest elevation and highest temperature, and in those whose plains are, by their height, covered with perpetual snow. The southerly face of these mountains, particularly in the valleys, is favourable to the existence of numerous animals which belong to hot climates: in ascending from these to the glaciers, a prodigious number of animals, vegetables, &c., are successively met with, peculiar to the temperature, zones, &c., which are traversed. The geological and mineralogical riches of the chain cannot be surpassed; every stratum being to be found in positions the most favourable for examination and remark. Finally; the students of this Pyrenean museum will not only learn to find, to collect, to prepare, and to preserve objects, but they will, under professors attached to the establishment and eminent for their acquirements, be taught to recognise them under all circumstances, to class them systematically, to seize accurately upon their characteristics, so as to be enabled to give such technical descriptions, as are now required from those who aspire to be historians of natural history, or even reporters of isolated facts relating to it.

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* M. Boubée is one of these public-spirited individuals.

"This course of combined instruction and practice at the establishment of St. Bertrand, will be entirely *gratuitous*. After two years passed in the Pyrénées, entirely occupied in travels, searching and collecting, and in the ateliers, preparing, preserving, and studying, the students, who may desire it, will be distributed, by the introduction of the directors of the establishment and according to their respective merits, among the various states, societies, and museums, who may have made applications for travelling-naturalists, preparers, and conservators, of skill, experience, and integrity."

We ought, in justice, to add, that the founders of this institution candidly state, that they do not profess to have, in creating it, no other interest, than the accomplishment of a philanthropic intention. They propose, as a remuneration of the necessary expenses, that the *results of the* researches and labours of *the students* shall be the property of the museum. This seems to be reasonable and fair; and even by this arrangement the general diffusion of knowledge will be assisted, for the various collections in all departments of natural history, which are prepared for sale at the museum, by the unpaid assistance of the students, may be disposed of at still lower prices.

It may also be useful to say, that a correct idea may be formed of the expense of residing at St. Bertrand, from the fact that the students of an ancient and excellent college still existing there, are boarded and lodged, including every expense, at about £20 per annum.

Taxidermy.

M. Gannal proposes a solution of the following salts for the preservation of animal substances, which from its cheapness and superior preservative qualities, seems to be preferable to the materials heretofore used.

Alum . . .	2 parts.
Chloride of soda .	2 ——
Nitrate of potassa	1 ——

Two dead bodies were immersed in a liquid containing these salts in solution, and at the end of two months were found to have undergone no change in their appearance. In general, the tissues and internal organs are perfectly preserved. Sometimes those immediately in contact with the fluid, lose their natural colour, but, further than this, no change takes place. The muscular fibres offer less resistance to pressure, than is usual, in a body forty-eight hours dead. It seems to be peculiarly well adapted for the preservation of the brain, as this organ, although thus kept for some months, will still serve for the demonstrations of the anatomist. This solution has also been successfully used as an injection in anatomical preparations.—*L'Institut.*

Stationary Temperature of Alcohol on heated Metals.

A curious fact has been observed in regard to the temperature to which alcohol of the specific gravity ·81, containing, therefore, 93 parts of absolute alcohol, and 7 of water, could be raised in a heated dish. It is necessary, as an introductory remark, to recall the fact, that when the temperature of a liquid is gradually raised, by applying heat to the vessel containing it, a limit is reached when the temperature of the liquid becomes stationary; the vapour given out in boiling carrying off the heat which enters the mass. When alcohol, of the strength above stated, was projected into a bowl, heated above the temperature at which repulsion of the fluid takes place, the temperature of the liquid did not rise to its boiling-point. In fact, the stationary temperature, instead of corresponding with that of ebullition, was lower as the temperature of the dish was higher.

Tabular View of the Manner, Cause, and Effect of the Destruction of Steam-Boilers, in the several cases in which it may occur.

DRAWN UP BY M. GALY-CAZALAT.

STEAM-BOILERS, (rectangular and cylindrical) may be	1. exploded	(if *large*), by an excess in the elastic force of steam, beyond the resisting power of the boiler, when developed slowly and gradually.	dangerous	1
		(if *large*), by an excess in the elastic force of steam, developed very rapidly or instantaneously.	dangerous	2
	2. rent . .	(if *small*), by an excess in the elastic force of steam, when the sides * are incapable of an equal resistance in all parts.	harmless	3
		(if *large*), by an excess in the elastic force of steam, when developed at a certain rate, and when the sides are incapable of equal resistance in all parts.	harmless	4
	3. crushed .	(if *large*), by pressure of the atmosphere, when the steam is sufficiently condensed.	harmless, or dangerous, according to circumstances	5
	4. perforated	(if *small* or *large*), by the contact of a foreign substance (a bad conductor of heat) with the boiler-bottom.	harmless	6
	5. displaced	(if *large* or *small*), by the inflammation of a certain volume of explosive gas in the fire-place and flues, when the former is closed.	dangerous	7
[N.B. The boilers under consideration in this Table, are supposed to be without tubes or partitions, internally; the fire and the flues to be external; the former beneath, and the latter around, them.]		(if *large* or *small*), by the instantaneous developement of a large volume of steam, generated by a sufficient quantity of water falling on a considerable surface of embers.	dangerous	8
		(if *large* or *small*), by the recoil produced by the destruction of the equilibrium of the internal pressure, in consequence of a large accidental opening.	dangerous	9

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* *By "sides," is understood (top-side, bottom-side, &c.,) the whole enclosing substance of the boiler.*

Railroad Mania in Germany.

THE municipal council at Chemnitz, in Saxony, were desirous of constructing a rail-road between that town and Dresden; but the mountainous nature of the intervening country rendering the execution of such a project extremely problematical, the authorities wisely determined on an efficient preliminary survey, the expense of which, amounting, as it was estimated, to sixty thousand francs, they proposed to defray by means of shares of two francs each; the holders of which were to be entitled to priority in taking those of the rail-road, if it were found to be practicable. When these experimental shares were publicly announced at Leipsic, a ludicrous scene of personal conflict with sticks and fists arose between the competitors, in their anxiety to secure the shares, and several persons who had announced their intention of taking two hundred shares could only obtain two, by great favour; so great was the competition for them, that, in a month, the two-franc shares sold for twenty, although no one yet knew whether the projected rail-road were practicable or not.

Bored Wells.

M. CASSIANO DE PRADO states, that at the city of Reuss, near to Tarragona, there are upwards of one hundred Artesian wells*, and upwards of sixty in the village of Villaseca; the supply of water is not only sufficient for the public fountains, for the purposes of irrigation, and to turn the mills in the neighbourhood; but it is even expected that a canal may be fed from these sources.

Gas from Peat.

GREAT advantages may be anticipated from the introduction of peat, in making gas for gas-light. First, it is less expensive than the gas from either coal, oil, or resin; second, the produce is nearly as much as from those substances: third, the gas is quite harmless and inoffensive, and has in respect to healthfulness, great advantages over the others; fourth, the peat, after having been used for the production of gas, may be used for fuel, and is equal to any charcoal.

According to the experiments of Merle, who is director of a gas company in France, one thousand kilogrammes of peat, when distilled like the stone coal, for two hours, yields eight thousand cubic feet of gas, which is of rather weak luminating power, and contains much carbon, and which, although apt to be purified by water, loses a great deal more of its strength; but if the same quantity is distilled for three-fourths of an hour only, five thousand and five hundred cubic feet of a pure gas are obtained, which is said to afford a stronger and whiter light, than coal or oil gas.

An apparatus, consisting of a condensator with eighteen tubes, is fixed for purifying the gas completely; each tube stands in a reservoir of flowing water, so that the gas has to pass eighteen times through the water, and is not deprived of its carbon; before the gas arrives in the large gasometer, it has to pass through two layers of dry lime; the gas thus purified, may be respired without any difficulty.

The construction of all other apparatus, may be made like that for other gases.—*Silliman's Journal.*

Lunar Influence on the Weather.

M. EVEREST, having observed that the greatest number of showers in the Spring occur at the new moon, has drawn up a table of the quantity of rain fallen in the first four months of the year for a period of eight years, specifying the number of rainy days before or after the change. The deduction from this table is, that most rain falls on the second, fifth, sixth, and seventh days before, and on the sixth after the new moon. By taking the *number of rainy days* during the same periods, instead of the *quantity of rain*, it appears that for the eight years, forty-five days were rainy in the quarter of the new moon, while the rest of the lunar month only gave twenty-three. An analogous difference, though not so striking was also observed during the two succeeding months of May and June, and the proportion approximated still more to a ratio of equality as regarded July; while for the remainder of the year the reverse law holds.

[It would be desirable if M. Everest's observations and deductions could be repeated for a longer period; there exist meteorological tables capable of furnishing data for the confirmation, or otherwise, of so interesting a question.—EDITOR.]

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* See Vol. I. p. 31.

Patent-Law Grievance. No. VI. The penalties inflicted on the inventive genius of Britain during the present year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £28,000!

N.B. This sum has been paid in *ready money*, on taking the first steps, and as many of the inventors are poor men, (operatives,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

NEW PATENTS. 1836.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

AUGUST.

185. Nathan Bailey, *Leic.*, Frame-smith; for improvements in machinery for manufacturing stocking fabric. Aug. 1.—Feb. 1.

186. John Thomas Betts, Smithfield-bars, *Lond.*, Rectifier; for improvements in the process of preparing spirituous liquors in the making of brandy. Aug. 3.—Feb. 3. *For. Comm.*

187. Webster Flockton, Spa-road, Bermondsey, *Surry*, Turpentine and Tar Distiller; for improvements in preserving timber. Aug. 3.—Feb. 3.

188. John Archibald, Alva, *Stirling*, N. B., Manufacturer; for improvements in machinery for curling wool, and doffing, straightening, piecing, roving, and drawing rolls, or cardings of wool. Aug. 4.—Feb. 4.

189. Ramsay Richard Reinagle, Albany-st., Regent's-park, *Middx.*, Esq.; for improvements in the construction of carriages for the conveyance of persons, goods, or merchandise. Aug. 6.—Feb. 6.

190. Thomas Binns, Mornington-place, Hampstead road. *Middx.*, Civil-engineer; for improvements in railways and in the steam-engines to be used thereon, and for other purposes. Aug. 6.—Feb. 6.

191. Thomas John Puller, Commercial-rd., Limehouse, *Middx.*, Civil-engineer; for a new or improved screen for intercepting or stopping the radiant heat arising from the boilers and cylinders of steam-engines. Aug. 9.—Feb. 9.

192. John Burns Smith, Salford, *Lanc.*, Spinner, and John Smith, Halifax, *York*, Dyer; for their method of tentering, stretching, or keeping out cloth to its width, made either of cotton, silk, wool, or any other fibrous substances by machinery. Aug. 10.—Feb. 10.

193. Henry Pershouse Parkes, Dudley, *Worcester*, Iron-merchant; for improvements in flat pit-chains. Aug. 11.—Feb. 11.

194. Joseph Douglass, Morpeth, *Northumberland*, Rope-maker; for improvements in the manufacture of oakum. Aug. 11.—Feb. 11.

195. Edward Light, Royal-st., Lambeth, *Surry*, Civil-engineer; for improvements in propelling vessels and other floating bodies. Aug. 11.—Feb. 11.

196. William Newton, Chancery-lane, *Middx.*; for improvements in the means of producing instantaneous ignition. Aug. 11.—Feb. 11. *For. Comm.*

197. Robert Allen Hurlock, Whaddon, *Camb.*, Clerk; for improvements in axletrees. Aug. 11.—Oct 11.

198. Joshua Butters Bacon, Regent's-sqr., *Middx.*, Gent.; for improvements in the structure and combination of certain apparatus employed in the generation and use of steam. Aug. 13.—Feb. 13.

199. Thomas Gauntley, *Nott.*, Mechanic; for improvements in machinery for making lace and other fabrics, commonly called wash machinery. Aug. 15.—Feb. 15.

200. George Leech, Norfolk-st., Islington, *Middx.*, Carpenter; for an improved method of connecting window-sashes and shutters, such as are usually hung and balanced by lines and counterweights, with the lines by which they are so hung. Aug. 15.—Feb. 15.

201. William Fothergill Cooke, Bellayse College, *Durham*, Esq.; for improvements in winding up springs to produce continuous motion applicable to various purposes. Aug. 17.—Feb. 17.

202. Joseph Hall, Margaret-street, Cavendish-sqr., *Middx.*, Plumber; for improvements in the manufacture of salt. Aug. 17.—Oct. 17.

203. François de Tansch, Percy-st., Bedford-sqr., *Middx.*, Military-engineer; for improvements in machinery for propelling of vessels, for raising water, and for various other purposes. Aug. 25.—Feb. 25.

METEOROLOGICAL JOURNAL FOR JULY, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month	Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer		Daily Temp	Solar Var.	Rad.	Clouds.		Wind.		Direction of wind		Lunation.	WEATHER, &c.
					Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Friday, 1	30·246	72°	30·240	76°	60°0	83°6	71·8	23°6	57°	5	4	0	1	E.W.	W.		Very hot; *cumuli* and haze; fine clear night.
Satur. 2	30·300	75	30·296	78	60·9	84·0	72·4	23·1	59	3	3	1·3	2	S.W.	W.b N.		Ditto; strong breeze P.M.; aurora borealis at night.
SUN. 3	30·352	73	30·336	74	50·0	80·1	65·0	30·1	48	4	0	2	1	S.W.	W.		Light clouds A.M.; afternoon cloudless and hot.
Mon. 4	30·351	74	30·325	79	53·2	86·3	69.8	33·1	50	0	0	1	0	W.	S.		Dew at sunrise; cloudless; lightning to W. and S. W.
Tues. 5	30·255	77	30·200	81	59·0	86·5	72·7	27·5	57	5	0	1	2	S.S.E.	E.	☾	Very hot; a breeze; continuous lightning all night.
Wed. 6	30·149	77	30·211	77	65·5	78.0	71·7	12·5	63	7	9	2	2	W.	N.		Thunder and lightning till 9 A.M.; cloudy; air
Thurs. 7	30·351	74	30·334	75	51·5	73·5	62·4	22·0	49	3	7	2	2	W.	N.N.W.		*Cumuli*; air cool. [cooler.
Friday, 8	30·382	72	30·395	74	55·0	73·8	64·4	18·8	53	5	3	2	1	N.	N.		Ditto ditto ditto.
Satur. 9	30·350	71	30·302	73	51·5	76·4	64·0	24·9	50	4	4	2	3	W.	W.		*Cirrus* and *cirro-cumuli*; a strong wind.
SUN. 10	30·256	72	30·246	76	58·6	82·6	70·6	24·0	57	7	3	2	2	W.	W.N.W.		*Cumulus*; much cloud.
Mon. 11	30·235	77	30·146	78	59·6	83·0	71·3	23·4	57	1	0	2	3·2	W.	W.S.W.		Hazy A.M.; strong wind; cloudless. [night.
Tues. 12	29·849	74	29·925	74	62·5	73·1	67·8	10·6	60	10	2	3	3	W.S.W.	W.N.W.		Overcast; a squall at 9 A.M.; a gale P.M.; clear
Wed. 13	30·158	72	30·100	73	49·6	72·0	60·8	22·4	49	3	2	3·4	3	W.	W.S.W.	●	Mostly clear; wind very high.
Thurs. 14	30·096	71	30·107	71	55·9	70·5	63·2	14·6	54	7	7	3	2	W.	S.W.		Windy; *cumuli* and *cumulo-strati*.
Friday, 15	30·015	70	29·858	70	50·0	69·0	59·5	19·0	47	10	7	3·4	3	S.W.	S.W.		Boisterous; a heavy shower at 8 P.M.; fine night.
Satur. 16	29·850	67	29·935	68	47·5	68·6	58·0	21·1	45	3	5	3	3	W.	W.S.W.		*Cumulus* and *cum.-stratus*; windy; thick cloudy
SUN. 17	30·035	66	30·100	68	56·2	68·9	62·6	12·7	53	8	5	3	3	W.b N.	W.		Clouds and wind; air dry and harsh. [night.
Mon. 18	30·245	66	30·200	68	53·0	70·0	61·5	17·0	50	4	3	3·4	2	W.	W.		Wind still blows hard; flying clouds.
Tues. 19	30·052	67	29·901	68	48·6	68·5	58·6	19·9	46	8	10	3	2	W.S.W.	W.		Overcast; blustering; rainy afternoon and night.
Wed. 20	29·745	64	29·561	64	49·0	55·0	52·0	6·0	50	10	8	1	2	S.N.E.	N.W.		Dark, cold, and wet till 2 P.M.; afternoon stormy;
Thurs. 21	29·795	64	29·774	65	43·4	64·0	53·7	20·6	42	7	8	2	1	W.	W.	☽	Thunder-showers; *nimbi*. [at 1 P.M. ther. 49°.
Friday, 22	29·900	64	29·916	64	46·5	61·6	54·0	15·1	44	8	8	1	1	W.	N.W.		Vast dense *cumuli*, *cum.-strati*, & *nimbi*; showers.
Satur. 23	30·157	63	30·182	64	49·2	68·0	58·6	18·8	47	5	5	1	1	N.N.E.	W.S.W.		Fair; a shower at 1 P.M.; fine evening.
SUN. 24	29·940	63	29·806	64	47·3	67·5	57·4	20·2	44	10	5	3	3	S. b W.	W.		Rain; wind very high; *cirro-stratus* and *scud*; P.M.
Mon. 25	29·905	63	29·998	64	49·5	64·0	56·7	14·5	49	9	9	2	2	N.W.	N.W.		Thick gloomy weather; cold & cheerless. [squally.
Tues. 26	30·229	65	30·226	66	49·7	72·5	61·1	22·8	48	4	10	1	2	W.	S.W.		Fair A.M.; afternoon cloudy; air soft and mild.
Wed. 27	30·261	67	30·254	70	59·2	74·0	66·6	14·8	57	8	7	2	2	S.W.	S.W.		Cloudy till 4 P.M.; a very fine moonlight night.
Thurs. 28	30·177	69	30·104	73	52·0	79·2	65·6	27·2	48	3	3	1	2	S.W.	S.W.S.	○	Hazy; *cirrus*; fine.
Friday, 29	29·814	70	29.700	72	59·4	71·0	65·2	11·6	58	10	7	4	4·3	S.S.W.	S.W.		Rain and wind; *cirro-stratus* and *scud*; afternoon
Satur. 30	30·075	66	30·248	67	53·2	63·5	58.3	10·3	51	6	6	3	2	W.	W.N.W.		Showery and cold; [and evening tempestuous.
SUN. 31	30·524	65	30·525	66	48·5	65·0	56·8	16·5	46	4	6	1	1	W.	W.		Fair; *cumulus*; evening overcast.
Mean	30·133	69	30·113	71	53·48	72·85	63·17	19·37									

Bar. Max. 30·524 in. on the 31st. || Ther. Max. 86·5° on the 5th. || Lowest point of Rad. 42°, on the 21st.
Bar. Min. 29·555 in. 20th. || Ther. Min. 43·4° 21st. || Rain fallen 1·915 in.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE BRISTOL MEETING.

In a former number of our journal will be found the details of the proceedings of that highly-interesting meeting which has recently taken place at Bristol,—the sixth anniversary of the important Association above-named. It is not our intention, therefore, in the following article, to offer any regular statement of those proceedings, but merely to throw out a few remarks of a general nature on the character and tendency of the institution, and on the degree of success with which it appears to us to have promoted the grand objects which it professes to have in view during its recent session, as well as to comment freely on what has struck us as defective in its arrangements. This latter part of our task (we need hardly say) is undertaken in no other spirit than that of the most sincere good-will towards this Institution; and with no other object than the earnest wish to contribute (if anything we can suggest may be conceived likely to do so) to the still further extension of its utility and efficiency;—to the yet wider diffusion and more effectual dissemination of those important benefits which it has already conferred, and is at present conferring, on the science of this country. We have on more than one occasion since the commencement of our journal taken occasion to uphold the claims of the British Association, and to defend it against the attacks of its enemies; and we are well aware it has neither few nor despicable opponents to contend with But we feel assured, that the mere advantage which those enemies possess against it, and of which they are never slow to avail themselves, are to be found, in fact, in some few defects under which the constitution of the Association has hitherto laboured; but which, we feel assured, its enlightened friends and supporters will not be slow to perceive or to amend.

The nature, objects, and claims of the British Association, are but imperfectly understood throughout the country. The men of science assemble; they prosecute their respective discussions, but forget that the public at large are remaining all the while uninformed as to the general purport and tendency of the objects for which they are assembled. *The Association does not explain itself to the public.*

The town in which the meeting is to be held, a few days previous begins to assume an appearance of unusual bustle. Strangers (some of them, it must be confessed, of strange and uncouth appearance) are seen in groups perambulating its pavements, uttering their greetings in the market-place, and discussing science at the corners of the streets. Anxious inquiries are heard from the ladies,—"Who is that genius with his hair about his shoulders;" and "Can you point out to me Professor Such-an-one." At length, on the Monday morning, work commences: the philosophers are all closeted in their respective sections. No one out-of-doors knows what they are doing; but all are led to believe that it is something extremely profound and important. At length, (after due honour done to the ordinary,) the grand epoch of the evening meeting arrives. The assembly-room, or the theatre, is crowded with anxious expectants, all looking out for amusement or instruction, as the case may be, for the gratification of their curiosity, or the indulgence of their risibility;—equally prepared to follow the most sublime abstractions, or to be entertained with whatever amusement philosophy may be capable of affording; but hardly any, perhaps, in the assembly, *having a distinct conception of the precise object which has brought them together.* Nor do the proceedings, (when, after due delay, they commence,) afford much elucidation on this head. The last year's president resigns the chair with a few words about honour and distinction, pride and happiness: the new president takes it with similar expressions of unworthiness and incapacity, of zeal and devotion to the cause of the advancement of science. Abstracts of the proceedings of the morning in each of the sections are read by the respective chairmen, which are for the most part wholly inaudible; and when they are not, are rendered so by the not unnatural preference given by the company to conversation rather than a string of technical nomenclature.

On the mornings of the other days, precisely the same course is pursued; and while some evenings are left open to be filled up by such amusements as chance may supply, on the others, the same general assembly takes place, the proceedings being so far diversified that some one or more scientific topics are discussed,—restricted usually to those of geology, and illustrated by that species of eloquence which so peculiarly characterizes the geological school, and tends to render its doctrines so singularly acceptable to the ladies. At the concluding meeting, little more is done to the purpose than at any of the preceding. What is wanting in philosophic exposition of truth is amply compensated, and at a cheap rate, by superabundance of flattery, and compliments, (not always such as we should consider the most happy or delicately imagined,) to the fair sex. Everything is hurry, apology, and abridgment; and the chairman dissolves the meeting with a reiteration of thanks and congratulations, and announcements of the coming glories of the next anniversary.

It may be asked, is this the language of friends to the Association? Is it the part of its admirers and supporters thus to expose its weakness? We reply that we wish to expose the defects in the working of the system, because we are sure that they admit of easy remedy, and that they have only to be pointed out and duly commented upon, to ensure the adoption of such remedy. We expose the weakness of the Association in these minor points, because we are assured that it has within itself the elements of gigantic strength, which only require to be called forth and developed, to evince its full efficiency in securing the great objects of its formation.

From what we have above remarked, it will be obvious, our first impression is, as to the necessity for *a more systematic course of public exposition of the objects of the Association.* At the opening meeting we would have the mere formalities as much as possible abridged. We would look to the president, or perhaps, rather to some one of the vice-presidents, for a luminous and popular explanation of the actual objects and views of the Association; which, as the meeting is held every year in a new place, and a considerable portion of the members is always new, would be no tedious repetition, but a most important and instructive exposition.

The address which has been annually drawn up by the secretary, reporting the progress of the scientific labours of the Association during the past year, has always appeared to us a most invaluable feature in the proceedings. This we are especially desirous to see kept up, and extended, and improved. We are not sure, whether, considering the high importance of this duty, and that we cannot always reckon upon having *such* secretaries as some of those who have hitherto filled the office,—it might not be a hint worthy of consideration, for *the* COUNCIL *to be charged with the production of such a report*, with express provision for putting it into the hands of some member who is able to do justice to it *in the delivery:* a point of increasing importance, as the assemblies of the Association are becoming yearly so much more numerous, and the places of meeting necessarily so large, that few speakers can be heard in them.

The reports of the proceedings of the sections very properly form an integrant and most essential feature in the proceedings of the general meetings. They are, in fact, the connecting links which now alone unite the distinct sections into the one great body of the Association. At the Oxford meeting the general assemblies were held in the middle of the day; and this arrangement rendered manifestly conspicuous the *unity* of the entire meeting. At these assemblies, the reports on the present state of the several branches of science were read: they constituted the main and essential portion of the whole to which the sections were but subsidiary; the main trunk from which the sections branched off. This arrangement has been discontinued. We are among those who deeply regret it, but we fear the general opinion of the members is against us, and that there is little probability of its being revived.

The present constitution of the Association is rather a *confederation* of distinct and isolated minor scientific states than one grand republic of philosophy, divided into subordinate districts. This we regret; but, as it is, we conceive it doubly important to keep up in an effective state that which now forms almost the sole semblance of the union;—the homage paid by the sections to their sovereign body and parent stock, even if it be little more than in form.

But we have already observed, this reading of the reports is on all hands, we believe, allowed to be (as at present conducted) the very worst feature in the arrangement of the society's proceedings. Yet it cannot be dispensed with. Indeed it is extremely important in all points of view, for the several sections have no other means of communicating their respective proceedings to each other; and the members of one section are anxious to know what has been doing in another; which, unless gifted with ubiquity, they can only do at the evening re-union. The only plan we conceive capable of adoption, is a systematic extension of that which has been partially followed at Bristol, and at some *previous meetings.* *In* one or two instances, eminent *individuals, connected with some of* the sections, have been called upon to come forward after the formal reading of the abstracts, and give some general and popular account of any material and striking investigations or discoveries which may have been announced at the section.

Now what we would hint, is, that this should be done regularly and systematically in reference to *all* the sections. With this view, let the formal abstract be reduced to the briefest possible limits, and let some member (not on the hazard of the moment, but duly selected beforehand, by the committee of the section) then come forward with a short, luminous, *condensed* popular sketch of the *most striking* parts of the proceedings, under the sanction of the *committee*, thus all invidiousness as to the selection of topics, and the estimate of the merits of the investigation would be done away.

In immediate connexion with this last idea, we further conceive it would be of great utility, if a general report of the progress made by *each section* during the *past year*, were regularly prepared some time previous to each meeting, under the immediate direction of the sectional committee, and handed in to the secretary of the meeting. Thus the details would be in a great degree supplied him upon which his annual report might be founded; these sectional reports might be printed entire in the society's annual volume, though not read at the meetings: they might, however, if thought desirable, be read at the opening of each section, in the same manner as the secretary's general report is read at the opening of the general meeting.

There is one extremely important point to which we wish especially to direct the attention of those interested (and we hope all our readers are so) in these meetings, we mean *the* EXHIBITION *of models, machines, and other objects of interest and curiosity.* The necessity for some provision for this object has been felt at every meeting hitherto, and we believe the means of supplying it have already formed the subject of serious discussion in the council. We believe it is in contemplation, at least we are assured it would be eminently deserving of consideration, to set apart some one large room, or repository, which should be open during the whole time of the meeting for the sole purpose just mentioned. Into this exhibition might be admitted (subject of course to the management of a special sub-committee), models and machines, intended to be described or referred to in communications to be given to the sections; or such as display any novelty of principle or improvement; the inventors of which may choose to attend and offer explanations of them in the room. Above all, opportunity would be thus afforded to the ingenious artisans *of the place* where the meeting is held, to bring forward their inventions, who would thus be led to feel an increased interest in the institution, extremely desirable to be excited, both for their own benefit and that of the Association. Nothing, we are convinced, would tend in a higher degree to the utility, as well as popularity, of the institution. It at present wants a connecting link with the practical and working men. Nothing would tend more effectually to supply the deficiency, than such an arrangement as that at which we have hinted. It would besides furnish a highly-agreeable resource to a great number of visiters, who may be little disposed to spend the whole morning in the more dry details of the sectional discussions. It would afford an agreeable promenade for the ladies, and we seriously think that everything which tends to make the meeting acceptable to them is really a point of no slight importance. We will not, however, dilate on this particular topic, however tempting, but we cannot quit the immediate subject of the *exhibition of arts* without referring to the practical argument in favour of it, which we have lately been enabled to appreciate in its full force, from attending the anniversary of the Royal Polytechnic Society of Cornwall, which (as our readers are aware) occurred at Falmouth, shortly after the Bristol meeting. This year's assemblage was distinguished by the presence of several eminent members of the British Association, and was, we believe, considered to have been an unusually brilliant and successful one. Those visiters, "one and all," (in the Cornish phrase,) agreed that the British Association could not follow a better model. The capacious room of this institution was filled with ingenious specimens of every description of works of art, as well as enriched by the exhibition of some most important experiments in the more abstract departments of science.

It is of course chiefly in the *former* points of view that we now refer to it. The display of mechanical genius (which in untaught and native exuberance, singularly abounds in this portion of the kingdom), was not only in the highest degree creditable and instructive, but (what is most to our present purpose) excited, for two successive days, the most lively interest among crowds, not only of the philosophic visiters, and the intelligent men of practical science connected with the mines, but also among the elegant groups in which all the beauty and fashion of the neighbourhood displayed itself; and, what is most valuable of all, among the working classes of the community, whose curiosity had not entirely abated even on the third day of the exhibition. We ourselves heard a ragged urchin in the street, exult that he had got a holiday to go to the Polytechnic.

We have already adverted to the importance we attach to whatever can increase the general *popularity* of these meetings. And assuredly one of the most material points in reference to this object, is the due supply of amusement blended with instruction, on those evenings on which there is no general meeting. Instead of leaving this to chance, or to the opportunities, however liberally given, by the throwing open of rooms or institutions, public or private, we would greatly prefer seeing this matter taken under the special superintendence of the Association itself, and made a regular and systematic part of its plan. Nothing seems so well calculated to effect this desirable end, as opening a large room solely for a *conversazione*, in which the parties collected might mingle in social intercourse or scientific discussion, as their respective tastes led them; and to this it would, perhaps, be by no means difficult to append the delivery of some popular lectures, perhaps in adjacent rooms, taking care that more than one are going on at the same time, to prevent undue crowding to one.

In thus throwing out our suggestions, it must not be imagined either that we speak in an obtrusive tone, as laying down the law to those *whose proper concern is the* government and regulation of the *meetings;—nor, on the other* hand, that we are promulgating these remarks as if from authority. We know not whether these, or any similar improvements, are likely to be actually adopted; we state them merely as our convictions, derived from the continued experience of all the previous meetings, and dictated by the most sincere good will towards the Association, and the most ardent wishes for its prosperity. We believe it is on all hands agreed, that the Bristol meeting was more important and successful in its *sectional business*, than any of its precursors: while, at the same time, it was less so in its public sittings. We understand the general impression as to the only portions of the public proceedings, which were other than mere forms (and those most irregularly conducted), was, that geology usurped the whole empire of science; and jokes (not the most decent) ruled over geology.

To take a single instance of want of management, without invidious selection, we will refer to Dr. Daubeny's lecture on mineral-waters and hot springs. The subject might be supposed likely to prove interesting and popular, but owing to the total unfitness of the *place* for such an exhibition, the diagrams were ill-seen, and the details were lost. Then, Dr. Daubeny having maintained, with his well-known ability, the doctrine of chemical decomposition as the source of volcanoes, instead of a central heat, he was opposed with great vehemence by Professor Sedgwick, and owing to the want of efficient previous arrangement, no one else was called up, nor were the general bearings of the case at all brought before the audience, who no doubt thought Dr. D.'s argument completely set down by his opponent,—nothing was elicited to show that it is a subject on which the arguments either way are almost equally plausible: and the term *central* heat must be purely hypothetical: *subterranean* heat we can readily allow, but not necessarily *central.*

In the sections, perhaps, the only difficulty is that of inducing those who come forward with communications, to abstain from lengthened details, and give the essence of their investigations in a more condensed form. This was strikingly exemplified in several papers read in the physical and mechanical sections. We observed the attendance in the former fall off greatly after the first day: we know not to what cause this was to be ascribed, except the one just referred to. The papers on the succeeding days were no way inferior in interest: but we believe complaints of the length of detail were very general.

The extremely important discoveries of Mr. K. W. Fox, and of Mr. Cross, on the electrical, or galvanic, action, continually at work in the interior of the earth, by means of which these gentlemen have shown, by actual experiment, the deposition of metallic veins, and the formation of crystals can be effected, were most properly brought before the society at a general meeting: but why then were not discoveries in other branches of science similarly stated? that is to say, why was not provision made for their being so stated? The valuable advances made in electricity by Mr. Snow Harris, would equally claim to be commented upon; as well as the extension of the undulatory theory of light by Professor Maccullagh; the motion of waves, by Mr. Russell; and several other important investigations which we could name. *A fair and equal share of attention to each branch of science*, and a selection of fit and able expositors *of each* (we have before observed), will, we trust, be a leading point of attention in the regulation of future general meetings. But the prevalent disposition seems to be very much in favour of abridging the proceedings of the general meetings, and we conceive no other way will be found at once more effectual to this end, and more conducive to the one just named, than a due arrangement for condensed statements of the most prominent results in each branch, from those best skilled in that branch, and best able to communicate their knowledge.

Upon the whole, we think it will be apparent, from what we have advanced, that our good wishes for the increasing efficiency of the British Association lead us to insist mainly on this general principle, the union of all branches, the centralization (to use a term of the times) of the departments, the public exposition of the leading discoveries, and the sectional discussion of the more abstruse principles, united into a more systematic combination. Add to this the admirable plan of an exhibition of the mechanical arts, and the opening of a regular *conversazione*, and we think, with these improvements, the Liverpool meeting will far outstep the advances of former sessions.

A POPULAR COURSE OF CHEMISTRY.

No. V.

Gases.

When speaking of "Chemical Affinity," at page 41, it was my object to show that a great many very beautiful and instructive experiments could be made with the common acids, alkalies, metals, salts, &c.; that a little caution was necessary in experimenting, but no very peculiar or particular management. By this I mean that the "chemicals" already spoken of are all *tangible* bodies, either *fluid* or *solid;* for instance, a certain quantity of *sulphuric acid* can be *poured* from a bottle into a glass, with as much facility as wine or water; and a piece of *potassa*, *silver*, or *pearlash*, can be *weighed* with as much facility as *tea*, *coffee*, or *sugar*. But there are a number of "chemicals" which are not such gross and tangible forms of matter. Many of these are perfectly *invisible*, of highly-attenuated and elastic natures: they cannot be *confined*, *measured*, or *weighed*, by ordinary processes. Yet the skill of the chemist has invented methods of operating upon them with as much ease and certainty as upon grosser forms of matter; and it is found that they are bodies as perfectly *material* and *ponderable* as any *fluids* or *solids*, although they are of an *aëriform* or *gaseous* nature.

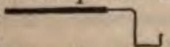
The department of science relating to manipulations with *gaseous* bodies is called *Pneumatic Chemistry*. The celebrated Dr. Hales was its founder, and the yet more celebrated Dr. Priestley its greatest improver; indeed, so indefatigable and ingenious was he in experiments and contrivances, that little room was left for any improvement upon his *pneumatic apparatus;* and as it appears in our laboratories at the present day, "we may consider it as almost entirely of Dr. Priestley's invention." *With the gases, then, we have now to experiment;* and here the young *chemist must be entirely* dependent upon his own skill and neatness of operating. It is true that *gases* can be *bought*, for in a "Catalogue of Chemicals" now before me, *oxygen* and *hydrogen gases* are arranged in "close column" with the grosser forms of matter, and respectively ticketted at "*sixpence per gallon.*" But he who would go to a shop for a gallon of "ready-made" *gas*, is *a philosophical gentleman* and not *a chemist*—there is a wide distinction to be drawn between these two personages. The one is struck with the splendour of apparatus, captivated with the neatness of a case of bottles, and enchanted with the idea of buying a gallon of ready-made gas, to save all dirt and trouble. The other cares none for splendour, and only for neatness, so much as is absolutely requisite for accuracy. He laughs at dirt, and trouble, and ready-made gas; and when he requires a gallon of oxygen, he fetches his own coals, lights his own furnace-fire, arranges his own apparatus, and feels a philosophic pleasure in watching the progress of the bubbling *element*.

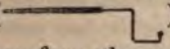
As a knowledge of the preparation and properties of *oxygen gas* is of the first interest and importance to the juvenile student, I shall now endeavour to put him in possession of the mode of operating with this *invisible gaseous body*: perhaps my details may appear too minute, but remember the motto,—"*Il faut savoir manipuler.*"

First then, as regards the apparatus necessary for the research. Considerable heat is requisite for the production of oxygen. Glass retorts "will not stand the fire," and iron ones of the common form are but very rarely met with. The following contrivance may be resorted to, and it will prove a useful and serviceable apparatus in many operations.

Have an iron gun-barrel, or what is even better, a piece of wrought-iron gas-tube, rather more than an inch in the bore, welded up close at one end: the smith can soon do this. If a gun-barrel, the breech and touch-hole must be the closed end: from 2 feet to 2 feet 6 inches will be quite long enough. This forms a sort of retort which is to contain the material for evolving oxygen.

This material is very abundant, and very well known as *black oxide of manganese*, or sometimes it is simply called *manganese:* four or five pounds will be required. It is a black powder, sometimes very coarse, and sometimes very fine; if the latter, it is apt to be damp, for it is *hygrometric* to a considerable extent, and requires to be perfectly dried before use. This is easily effected by spreading it on the hot hob of the grate, or heating it gently on a shovel held over the fire. If a small pane of cold window-glass be now held over it you will find the glass instantly dimmed on its lower surface, in consequence of the *vapour* of the water or *steam* rising from the manganese, and being *condensed* by the cold surface of the glass: stir the manganese with an iron-wire, so that all parts of it may get their full share of heat; *test* for water every now and then with the glass plate, and when it remains bright and clear you may be sure that the manganese is properly dried and fit to be removed from the fire. Sweep it from the hob, or pour it from the shovel on to a plate of tin slightly bent in the middle, as thus which is a convenient form for pouring the powder into the iron tube, *this must be done* until it is about half, or rather less than half full.

The *manganese* must be poured in *lightly* and not *rammed* down, and its quantity in the tube can be easily ascertained by a cane or stick thrust down from time to time. Having proceeded thus far, the next step is to fit a good sound cork to the end of the tube: a hole is required through the cork, which is very easily and neatly made, first with a brad-awl, and then enlarged with a "rat-tail file," until the end of a half-inch pewter pipe, about 2 feet long fits tightly into it. A strip of bladder, well soaked in water until it is quite soft and gelatinous, must now be put round the iron tube, near the cork, and neatly and smoothly made to surround the cork, and part of the pewter tube: bind it round with string, and thus a capital *gas-tight* joint is effected; and the next thing to be done, is to bend the pewter tube. Suppose the broad line between these parentheses ( ) to represent the iron, and the narrow line the pewter tube. The latter must be bent so as to form this figure (). The object of doing this will appear presently; and now the apparatus for *evolving* oxygen gas is complete: the apparatus for *collecting* it remains to be described. This is very simple and unexpensive. A tub, pan, or trough of wood or metal, about the size and shape of a "foot-pan," (and, indeed, those neat japanned "foot-pans" now in fashion, are excellent for the purpose,) must have a tin or wooden shelf, about 6 inches wide fixed right across it at one end, about 2 inches down from the top of the tub or pan, and in this shelf must be a hole $\frac{3}{4}$ of an inch in diameter: this constitutes the *pneumatic trough*, which is to be filled with water, so that the shelf may be an inch beneath its surface. The next thing will be to provide some pint or quart wide-mouthed glass bottles, with ground-glass stoppers, which are to be lightly smeared on the ground parts with pomatum or tallow: this is best done by wiping the neck and the stopper both perfectly dry; and then taking a bit of pomatum or tallow on the point of the finger, and applying it to the stopper, which if then put in its place, and turned gently round once or twice, will distribute the grease around the neck of the bottle: this is another *gas-tight* joint, and now we are all ready to begin work.

The end of the iron tube containing the *manganese* is to be thrust horizontally between the bars of the fire-grate, so that the pewter tube hangs down, as thus (). In a few minutes, perhaps, a steamy smoke will issue from the pewter tube: this is due to a little more water expelled from the *manganese* by the higher degree of heat to which it is now exposed.

If the *manganese* was not dried at the outset of the experiment, much more steam would, of course, issue; and another thing is very likely to happen, which is this:—

Supposing the manganese damp, and the iron tube containing it set in the fire, sloping slightly *upwards*, the steam would *condense* into drops of water at the other end, which is yet cold: and if several of these coalesce into one large drop, this would trickle down the tube, and meeting with the iron and manganese, both very hot, the drop of water would *be suddenly* and *instantaneously* converted into steam of such *force as to blow out the cork*, and all the contents of the bottle.

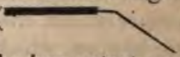
This accident is not uncommon with juvenile operators, and it has more than once befallen the experienced chemist, before he was aware of its cause. Dr. Faraday nearly lost his sight on two occasions by the hot manganese being thus driven into his face; and these accidents induced Mr. Griffiths to investigate the hygrometric powers of manganese and other solid bodies; an account of which will be found in the *Journal of the Royal Institution*; but by drying the manganese the accident is wholly avoided.

The steamy smoke just spoken of will soon cease, and then is the time to set about detecting the presence of *oxygen gas.* Light a paper "allumette," and hold it at the end of the pewter pipe; you will find the flame become very brilliant: this is one *test* of the presence of *oxygen.* Blow out the flame, and apply the glowing paper-cinder to the pipe: it is instantly *rekindled.* This is another *test*, and shows that some peculiar *gaseous* matter is now evolving and escaping: this has to be collected.

Bring forward the *pneumatic trough*, and place it so that the bent pipe dips below the surface of the water; and the instant that this is done, a bubbling of the gas takes place through the water: shift the *trough*, so that the tube may be exactly under the hole in its *shelf;* the *gas* now begins to come over more plentifully, for the *retort* is getting hotter.

In order to collect it, fill one of the bottles *completely full of water* in the *trough*, and then turn its mouth *downwards*, and raise up the bottle gently and carefully until its mouth rests on the *shelf;* slide it carefully along. No water can run out of the bottle, because *atmospheric pressure* keeps it in; but as soon as you slide the mouth over the *hole*, you will find the bubbles of *gas* rise rapidly into the bottle, displacing the water. Why does the gas thus rise through the water and collect in the bottle? For the very same reason that a *cork* rises through *water*, because it is *lighter* than water. If you fill another bottle with water, keeping its mouth below the surface, you may fill the bottle full of phial-corks, by putting them one by one under water, so as to rise into the bottle; each *cork* represents a *bubble of gas.*

Now, when the bottle is quite full of *oxygen gas*, you must slide it gently off the shelf, with one hand, into deep water, and with the other put in the stopper, giving it a slight twist, and then you may take out the bottle, *full of gas*, and place it aside in any position; the gas is safely *collected and confined*, but being *invisible*, any person would call the bottle *empty.*

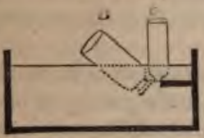
Two or three bottles may be thus filled and removed in succession; at length, however, all evolution of gas ceases, and then the *trough* must be removed, and the pewter pipe bent as thus (), in order to let any water drain out of it; lay hold of the iron retort, near the pewter pipe, with a double worsted glove, draw it from the fire, lay it on the hob of the grate, letting the pewter pipe hang down;—now, if you attempted to draw out the retort without bending down the pewter pipe, the chances are, that you might accidentally turn it round in the hands, so as to allow the water contained in the angle of the pipe, to run into the red-hot retort, and of course you would have an explosion; but bend it down as I have directed, let it drain awhile, and all is safe.

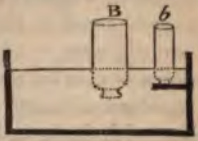
I have said that any person would think that your bottles *full of gas* were all *empty* bottles, and perhaps might open them, and thus destroy your labour by permitting the gas to escape. To prevent this, as well as to guard against any accidental expansion of the gas forcing out the stopper, it is as well to tie them over with a bit of cloth or calico; and the gas may thus be kept for many months, in a cool situation.

But supposing one of the bottles holds a quart of gas, and you should like to fill a smaller bottle from it, how is this to be done? Certainly not by the common mode of pouring, as with wine or water; at least, it would require more practical skill than you have yet attained; but there is a very simple and easy method of doing it, as follows.

Fill the smaller bottle *quite full* of water in the *trough*, and slide it on the shelf as before; now take the large bottle, and plunging its neck *quite* below the water, withdraw the stopper.

Suppose these lines to represent the trough and the bottles: B is the large, and *b* the small bottle, the faint line is the level of the water, the dark lines the section of the trough; with one hand slide the small bottle nearly off the *shelf*, and with the other incline the large bottle into this position, beneath its mouth: the consequence will be, that the gas from B will bubble up through the water, and fill *b* to any extent that you please,—say quite full,—it is then to be slid on to the shelf, B restored to the perpendicular, with the *mouth* still *under water* remember, the stopper replaced, and then remove the bottle from the trough. Now you have to attend to *b;* slide it off the shelf into deep water, and put in its stopper as before; and thus by *inverted pouring*, if the expression may be permitted, an invisible gas is *transferred* from one vessel to another.





It must be obvious to you that as much water enters B as *b* contained; and therefore a corresponding quantity, or *bulk*, of gas from B has entered *b*. In order to gain dexterity in gaseous manipulation, it is a very good plan to practise transferring common air from one bottle to another; for perhaps at first you may waste a good deal of gas.

Bottles and jars unprovided with stoppers may be filled with gas in the way that has been described, and when these are to be taken from the trough, it is easily done by sliding them into deep water, placing a saucer beneath their mouths, and thus removing them, standing in the saucer, the water remaining in which effectually prevents the escape of the gas, and is called a *water-lute*. If it is desired to transfer the gas from one of these jars at any future time, it must be brought to the trough, sunk a little below the surface, and then, upon removing the saucer, the jar of gas may be operated upon at will.

For the experiments about to be mentioned with oxygen, stoppered *bottles are the most convenient.*

The first and most remarkable property of oxygen, is its power of *supporting combustion* in a far more energetic manner than common air. This was hinted at above, when directing you how to *test* for its presence; but now that a considerable quantity of the gas has been collected, this fact can be shown in a still more remarkable degree.

Stick a bit of green wax-taper at the short end of a wire bent like this , light it, open one of the bottles of oxygen in the usual way, that is to say, as it *stands* on the table, and quickly plunge the taper into it; the flame is greatly enlarged, and the light is most brilliant; draw it out, puff out the flame, and again dip in the glowing wick; it is rekindled with a sharp pop or explosion. There is no fear of oxygen escaping whilst you do this, for its *weight is greater* than that of air, and therefore it remains in the bottle when opened, because the *lighter* air cannot *descend* and force it out; just the same as water, which is heavier than air, remains in a glass.

I cannot enter into the details about ascertaining the weight of *oxygen;* but perhaps the following example will render the matter intelligible. Supposing that I have a glass vessel, *exhausted*, or *devoid* of air; that I take the *tare* of it, and then allow *air* to rush in and *fill it;* upon weighing again, I find that it has increased in weight 1000 parts. This, therefore, is the weight of *air* which the vessel can contain. I now remove the air, leaving the vessel as empty as before, and then allow *oxygen* to rush in and *fill it:* upon weighing again, I find that it has increased in weight 1111 parts. This, therefore, is the weight of a *bulk* of *oxygen* equal to the former *bulk* of *air;* the *bulks* are *similar*, the *weights* are *different*.

Oxygen is *heavier* than *air*, or, as chemists express it, thus: calling *air* the standard of unity, or =1·000, the specific gravity of *oxygen*, in comparison with it, is =1·111. Now this little difference in weight is amply sufficient to cause it to remain in the bottle for some little time after the stopper is removed: it is true that it will escape out of the bottle in time; but this is not on account of the light air descending and forcing it out, but from a singular tendency which gaseous or aëriform bodies have to mix with each other, independent of all reference to their relative weights or specific gravities; a case somewhat analogous to that of the mixture of *spirit* and *water*, mentioned in the paper on *Chemical Affinity**.

Although *air* cannot fall through *oxygen*, yet *oxygen* can fall through *air*, as may be proved by *inverting* a bottle of the gas, and removing the stopper. After the lapse of a minute, if you put a lighted taper into the bottle, it will burn with its usual flame. If the air of the room is very calm, and you take a wide-mouthed bottle of air, and set it on the table, you may perhaps succeed in *pouring* it full of *oxygen* from one of the bottles that has been collected. A lighted taper will soon show whether you have succeeded, for it will burn with brilliancy in the bottle that just now contained the *air*, but with its usual flame in the other from which the *oxygen* has poured.

I shall hereafter adduce some very remarkable experiments of this nature with other gases.

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* See *Magazine of Popular* Science, vol. ii., p. 41.

Oxygen gas is without taste or smell: it has no action on vegetable colours. Put a bit of *litmus* and a bit of *turmeric* paper[*] into the gas; neither of these *tests* are affected, and therefore *oxygen* is neither *acid* nor *alkaline;* and yet the name would lead you to imagine that it possessed some relation to acids. Let us see how this can be made out.

Take a little bit of *phosphorus*, about the size of a small pea[†], wash it in some clean water, and then place it on a bit of *litmus-paper*, it shows no signs of acidity, now quickly and cautiously wipe it dry, and place it in a small ladle, called a "deflagrating spoon," the wire of which is thrust through a cork, or "shive," large enough to *cover*, but not to fit the neck of a bottle of oxygen, the part of the cork next the rim of the bottle should have a circular flat disc of tin-plate upon its surface, for this is a combustible experiment, and the flame of the *phosphorus* would soon set fire to the uprotected cork, and, perchance, break the bottle; the disc of tin acts as a screen, and prevents this taking place.

Having arranged this matter, and by sliding the wire through the cork, adjusted it so that the spoon may hang about half-way into the bottle, which you can guess at by outside measurement; the bottle is to be placed on the table, its stopper loosened with one hand, whilst the other holds the spoon over the flame of a candle, until the *phosphorus* first burns. The stopper is then to be withdrawn, and the spoon with the burning *phosphorus*, quickly, yet steadily, plunged into the gas.

A most splendid combustion instantly takes place, intense heat and light are evolved, in consequence of the *mutual affinity* which the bodies have for each other, and when the combustion is over, you will find the bottle filled with dense white fumes; remove the spoon, pour in a little water, close the bottle lightly with a cork, and leave it at rest for a few minutes. You must not close the bottle with its own stopper, because, during the combustion, the contents of the bottle are rarefied and expanded by the heat, and if the ground stopper was directly put in, as the bottle cooled, the pressure of the atmosphere would fix the stopper so firmly in its place, that it is a chance whether you would ever be able to move it again. This should be remembered in all analogous experiments, for many a valuable bottle is thus rendered useless, or perhaps broken, by attempts at forcing out the stopper.

Now put a bit of *litmus* paper into the bottle; its *blue* colour is intensely *reddened;* here then you have an instance of *chemical affinity*, producing an *acid*, viz., the *phosphoric acid.*

Sulphur, *carbon*, and several other simple substances, will burn with considerable splendour in *oxygen*, and produce *acid compounds.* So general, indeed, was the *acid* result, that *oxygen* was at one time adopted as the universal acidifying principle, and hence the derivation of its name. We now, however, know that it is equally active in producing *alkalies* (which are diametrically opposed to acids), and also bodies, having neither *acid* nor *alkaline* properties, *viz.* metallic *oxides; black oxide of manganese*, for example.

Place a small globule of *potassium* in a deflagrating-spoon, heat it over a spirit-lamp, until it begins to burn*, and then plunge it into *oxygen*, a vivid combustion ensues; wash out the spoon with a little water, dip in a bit of *turmeric-paper*, the *yellow* is instantly changed to *brown*, indicating the formation of the *alkali potassa*, or the *alkaline oxide of potassium.*

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[*] See *Mag. Pop. Science*, ii., 47.

[†] *It is very combustible, and must be handled with great caution, for the heat* of the fingers will often kindle it; and, therefore, you must keep it under water until the moment that you want it.

If you take out the *manganese* from the retort, insert a fresh charge, and proceed to evolve oxygen again; another experiment can be made, which is highly instructive: take a large bladder, soak it well in water, tie on to its neck a common gas stop-cock that will fit the end of the pewter pipe, and as soon as oxygen comes off, squeeze the bladder, so that no air remains in it, then fix it on to the pipe with the cock open. If the cock does not screw on, you can make it do so, by putting some paper between; but it is much better to fit it with paper so as to *slide* on, because when the bladder is full of gas you can instantly remove it by sliding the cock off the pipe—screwing it off is not so handy. When the bladder is full of gas, shut the cock, and quickly remove it from the end of the pipe.

Another bladder may be filled in like manner:—to the stop-cock of the first, fix on a plain brass blowpipe (which can be easily procured at the ironmongers'), and make the joints tight with slips of wetted bladder as before directed.

Now take a piece of charcoal, about six inches long and an inch in diameter, make a small cavity in it with the point of a knife, big enough to hold half a pea, put a bit of lighted *amadou* in this, and by opening the cock, and gently compressing the bladder, force a stream of oxygen slowly from the beak of the blow-pipe upon the tinder, this will soon heat the cavity of the charcoal red-hot; and when this happens, instantly drop in a bit of steel, broken from the end of a file, or a cast-iron "sparable" will do:—urge on the gas, an intense heat results, the metal melts, and presently *burns* with the emission of a shower of brilliant sparks, exactly like the celebrated fire-work, called a gerbe; when these cease, shut off the gas, let the globule cool, and then examine it: you will find it very brittle and easily reduced to powder. Try it with the test-papers. It is neither *acid* nor *alkaline*, it is a *neutral metallic oxide of iron.*

Now these three experiments are very instructive ones, they show you intense chemical affinity existing between bodies of the most opposite natures, they show you *oxygen*, forming an *acid*, an *alkali*, and an *oxide*, all again new and distinct substances; they also show you how energetically oxygen *supports combustion*, and about the time that oxygen was adopted as the "universal acidifying principle," it was also adopted as the "universal supporter of combustion." This was another grand error, for we now know that there are instances of combustion in which *no oxygen*, or compound of oxygen, is present, and of others in which its *presence* so far from *inducing*, actually *prevents combustion;* but it *takes*

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* The potassium must be wiped free from the *naphtha* in which it is usually kept, and when you hold it over the spirit-lamp, very likely you will see a flame like coal-gas quickly rise from the globule, this is the naphtha burning off; and you must not plunge it into the gas until a *purple* flame appears; that is, the flame of potassium.

place the moment that *oxygen is withdrawn.* At the time of the discovery of oxygen (1774), philosophers had hardly shaken off the trammels of alchymy, and it was the fashion to draw sweeping and general conclusions from a few experiments; at the present day, our operations and reasonings are conducted with more refinement and precision; hypotheses and theories are carefully examined before they are so generally adopted as heretofore. Philosophers now endeavour to act up to the Baconian precept, which says, "Conclusions are, in all cases, to be drawn after the comparison of a sufficient number of facts, with a due regard to objections."

Oxygen is an *elementary* substance, permanently *gaseous* at all known temperatures and pressures: it is most abundantly distributed throughout the three kingdoms of nature, but always in *combination* with other elementary substances; it has never yet been found in a *free* or uncombined state.

The black *oxide of manganese* is the *metal manganese saturated with oxygen*, and therefore a *peroxide*, (see vol. i., p. 298,) consisting of *manganese* 28 + *oxygen* 16 = 44; when this is heated red-hot, the affinity between the two substances is weakened to some extent. Part of the oxygen escapes in the *free gaseous* form, leaving a *sesquioxide* of a brownish colour, consisting of *manganese* 28 + *oxygen* 12 = 40 *sesquioxide of manganese;* and in this compound the affinity between the two substances is so strongly exerted, that heat alone cannot overcome it, therefore no more oxygen can be extracted from the *sesquioxide.* This, however, must not induce you to throw it away as useless; it should be preserved for some experiments hereafter to be mentioned.

Oxygen supports respiration: Priestley put a mouse into a jar containing it, and he found that the animal lived about thrice as long as when confined in an equal bulk of common air.

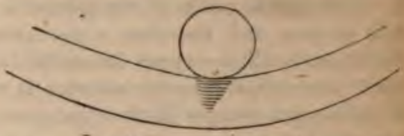
You must not conclude from this, that it is eminently fitted for maintaining the functions of vitality, for the contrary is the case; an animal caused to breathe pure oxygen for any length of time, at last falls a sacrifice to its stimulating agency, and upon examination after death, the blood in the *veins* is found as *florid* as that in the *arteries.*

It is well worth notice and recollection here, that although we have many gases that may be breathed for a considerable time without hurting life, yet we have *no gas or mixture of gases* fit for its perfect support, save *atmospheric air*, which you will hereafter find to be a *mixture* of *oxygen and nitrogen.*

EXPERIMENTS ON THE STRENGTH OF IRON BEAMS.

The experiments of Mr. Eaton Hodgkinson, of Manchester, are said to have led to a practical economy of material in the construction of the great iron girders or beams, so generally used, there and in other manufacturing districts, in the building of factories, amounting to not less than 20 per cent. of their weight.

It is very rarely that the application of principles of exact science to the uses of society is attended with success like this. And we feel that the pages of this magazine cannot be occupied more in accordance with the purposes for which it was established, than by giving publicity to these experiments.



If a beam of iron, or any other elastic or flexible material, be bent by a weight which it supports, it is manifest that the part of it lying near that side which supports the weight, will in the act of flexure be *compressed*, whilst that on the opposite side will be extended*. It is at that part of the beam which is nearest to its extended side that the extension is greatest, and at the part nearest to the compressed side that the compression is greatest. Between the point of greatest extension, and the point of greatest compression, the extension diminishes continually up to a certain point, where it is nothing; and beyond that point the compression commences, and continues to increase up to the other side of the beam where it is greatest. The point of the beam where the extension of its material terminates, and the compression of it begins, and where there is, therefore, neither extension nor compression, is called its neutral point. It is not at one point only, however, of the beam, that this neutral state of its compression and extension exists, but manifestly throughout all the points of a line crossing the whole width of the beam, and passing through the neutral point. This line is called its neutral axis.

The forces which oppose themselves to the rupture of the beam are the resistance of its material to extension on one side of its neutral axis, and to expansion on the other. Its power of resistance to *either* of these yielding, it will be broken. Thus, if the one side be so far extended that its material separates, the beam will fail, although the other side may still retain its power of resistance to compression. Or if the one side be so far compressed that it crushes, the beam will fail, although the other side is still able to resist the extension to which it is subjected.

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* This may be seen in a very simple experiment. Let a piece of deal be gradually bent, and the part where the principal flexure takes place observed. It will be plainly seen that, on the side from which the flexure is made, the fibres elongate, and that on the other side they *compress; and when a* complete fracture has been made, this process will be further indicated by the appearance of the broken ends which will on one side be jagged, indicating there a rupture of the fibre by tension or tearing asunder,—and on the other side, comparatively smooth, as they would be if compressed.

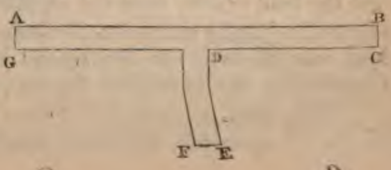
In the first case, the beam opening on the extended side, the compressed side would form a fulcrum, about which the separated part of the extended side would turn.

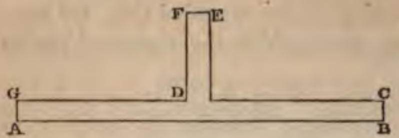
In the second case, the compressed side of the beam immediately beneath the weight, being no longer capable of resisting the compression to which it was subjected, would be crushed in pieces, or otherwise displaced, and the beam entirely broken; although, perhaps, the tensile resistance of the opposite side of it had never yielded.

This power of resistance to compression, and the power of resistance to extension, constituting the strength of the beam; and the yielding of either of these being of necessity followed by its entire rupture, it is manifest that the material of which the beam is composed will be distributed so as to make it the strongest when it is so distributed, that the one side shall be about to yield by compression, when the other is about to yield by extension. For, if either rupture is about to take place when the other is not about to take place, a portion of the beam might be removed from the stronger side, without causing that side to be in a state bordering on rupture, and added to the other side, so as to take that out of the state bordering on rupture. And thus if the powers of resisting compression and extension be unequal, the strength of the beam may be increased by a new distribution of the material.

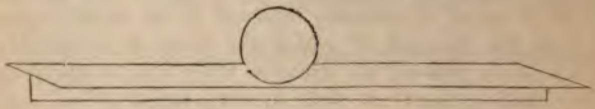
This being admitted, the question of the best form of the beam resolves itself into this:—How can the material be distributed on the two sides of it so that the resistance to the compression to which the one side is subjected, and the resistance to the extension of the other may be equal? A principal element in this inquiry is manifestly this:—Is the power of a given quantity of material to resist compression the same as its power to resist extension?—if it be the same, it seems probable that the object would be gained by any arrangement by which the part which is subject to compression should be made exactly equal and similar to that which is subject to extension. It appears, however, from the experiments of Mr. Rennie, that at any rate in respect to cast-iron, this law does not obtain. These experiments, and others of the same kind, which had before, and have been subsequently made, show very clearly that the cast-iron will resist a much greater force tending to compress it, than it is able to resist when the force tends to extend it. And that thus to produce an equal power of resistance on the two sides of the beam, a larger quantity of material should be collected on the extended than the compressed side. This idea suggested itself first, it appears, to Mr. Hodgkinson; and he contrived the following ingenious experiment to serve as a verification of it.

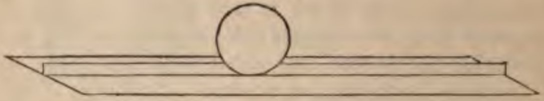
He caused two castings to be made, 5 feet in length, and whose cross-section was of the form represented in the figure; the width, AB, being $4\frac{1}{10}$ inches, the depth of the rib, DE, $1\frac{1}{10}$ inches, and the *thickness, BC, of the metal throughout $\frac{1}{4}$ inch. Now, it is manifest*





that this casting being placed in the position shown in the first the accompanying figures, with rib downwards, and being load the portion, ABCG, of the sect of fracture would be subjected compression, and the whole, or lower part, of the rib DEF would subjected to extension. Moreov the surface, AGCB, resisting the force of compression, being so mu greater than the rib DEF, which opposes itself to the force of extension, is clear that when the casting yielded, it would, under these circumstanc yield by the extension of DEF. Again, if it were placed as in the seco figure, with the rib upwards, and loaded in the middle, the compres portion of the section of rupture would be the rib, FED, or the up portion of it, and the extended portion ABCG. The surface sustain the forces of compression would, therefore, in this case, be less than t sustaining the forces of extension; nearly, perhaps, in the proport in which the area, EFD, is less than ABCG: and if FED were sufficie small as compared with ABCG, the rib would of necessity yield to forces which compress it, before the part ABCG yielded to the for which extend it. Thus, in both cases, the casting would break by yielding of the rib, EFD. But, in the first case, it would yield by extension of that rib; and, in the second, by the compression of it. N it was found that the disproportion between ABCD and FED was in th castings sufficiently great to produce these results, *i.e.* to cause the used in the second experiment to yield to the compression of the whilst that in the first yielded by its extension.



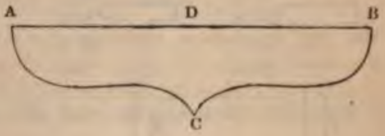


If, then, it be true that the same material in a beam yields m easily when it is subjected to extension than when subjected to c pression, the casting ought, in the second case, when the rib was c pressed until it broke, to have borne a greater weight than in the fi when it was extended until it broke.

In each experiment the supports were placed 4 ft. 3 in. asun and the load exactly over the middle point. In the first case, when rib was broken by extension, the beam just bore $2\frac{1}{4}$ cwt., and the break load was $2\frac{1}{2}$ cwt. In the second case, where the rib was broken compression, the beam bore $8\frac{3}{4}$ cwt., and was broken by 9 cwt.

Thus, then, a beam of this form and these dimensions, when tur *with the rib* upwards, will bear nearly four times as much as when pl with the rib downwards, and its rib requires four times as great a power to break it by compression as by extension.

The weights in the last experiment were very gradually laid on, and no rupture of the material could be perceived until the instant of fracture. A wedge then flew out of the compressed side, of which the form is accurately represented in the figure. Its length, A B, which was in the direction of the length of the casting, was 4 inches, and its depth, C D, ·98 inches. This depth, probably, indicated the whole depth of the compressed portion of the section of rupture, which was, therefore, very nearly that of the rib*.



These experiments sufficiently indicate the strength gained by accumulating the material of the beam on that side of it which is subjected to extension; and they at once suggest the inquiry, what should be the amount of this accumulation? It has been shown, that the strongest form will be obtained when the material is so distributed, that it may offer the same resistance to the forces which, on the one side tend to compress it, as to those which, on the other, act to extend it. And moreover, it is now shown that less material is requisite to effect the first object than the second. The aim of the remainder of Mr. Hodgkinson's experiments, was to determine in what proportion it should be less.

Before, however, entering upon this investigation, a very simple improvement in the form of the casting suggested itself to him. Whether the beam was about to break by the separation of the extended part, and the turning of the fractured portions about the compressed part as a fulcrum, or by the yielding of the compressed parts, and the turning of the two ends about the extended part as a fulcrum. It was manifest that the forces which opposed themselves to the fracture would in either case be most effective when they acted at the greatest distance from what would in that case be the fulcrum.

Thus, then, that form of the beam would be best which placed the material which was to resist compression at the greatest distance from that which was to resist extension, or which collected the material on the upper and under sides of the beam.

This principle characterizes the forms of the sections of all the castings used in the following experiments. They were each $5\frac{1}{8}$ inches in depth in the middle, and broken between props 4 feet 6 inches apart.

The general form or elevation of each, was that represented in the first of the following figures; and the forms of their middle sections were in the order of those shown by the diagrams below, which are each one-fourth of the real size of the section.

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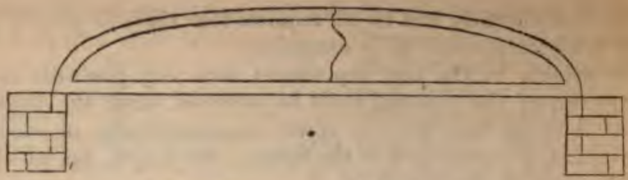
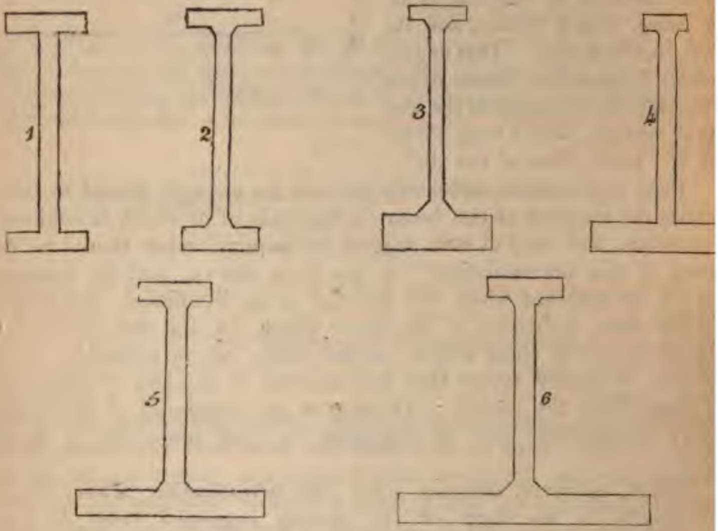
* The form of the wedge was remarkably regular, and it preserved its regularity of form, and the same dimensions, in a variety of other similar experiments subsequently made. *We know, as yet, too little of the mechanical construction of bodies to be able to give any explanation* of the form of this wedge. The subject will, however, possibly not be found without the reach of analysis, whenever the highest resources of that master-science shall be applied to the theory of the strength of materials.





It will be observed that in the first of these the portions of section subjected to extension and compression were of the same mensions; and that in the second, the portion subjected to extens was greater than the other; and in the third greater still, and so until in the last, the portion resisting compression was exceedingly sm as compared with that opposing itself to extension.

Now, from what has been said before, it is manifest that in first the material opposing itself to compression is in *excess*, and th portion of it might be removed with advantage, and added to the lo portion of the section. This is done in the second experiment, and a yet greater degree in the third and the fourth, &c. We may theref expect that the beam would thus be continually strengthened up t certain point, when the compressed portion would have become so sm as to yield before the extended portion; and thus, if the gradations sufficiently slow, the precise form under which the compressed and tended portions equally resisted the forces to which they were subjec that is, the *best* form of the section, would be ascertained.

Now, the best and simplest method of comparing the strength beams of different sections, is probably to ascertain the weights in pou necessary to break them, and to divide this by the number of squ *inches in* the section of fracture of each; the quotient may be und *stood to be* the number of pounds of strength supplied by each sq *inch of section;* and that form of section which thus supplies the gr number of pounds of strength per square inch to be the strongest. Adopting this mode of comparison, the result of these experiments will appear from the following table.

No. of Experiment*.	Ratio of Surfaces of Compression and Extension.	Area of Section in Inches.	Strength, per square Inch, of Section in lbs.
1	1 to 1	2·82	2368
2	1 to 2	2·87	2567
3	1 to 4	3·02	2737
4	1 to 4½	3·37	3183
5	1 to 4	4·50	3214
6	1 to 5½	5·0	3346

All these castings were made with iron, of which the following is the description:—

	Mixture.	
⅓ of Blaina	No. 2,	Welsh.
⅓ of Blaina	No. 3,	
⅓ of W. S. S.	No. 3,	Shropshire.

In every case, the casting broke by the yielding of the extended or lower portion of the section, and in all, except the fourth and fifth, there was indicated a continual increase of strength, as more of the material was accumulated in the lower portion of the casting. In the fourth, it was believed that the upper flanch, or rib, had been so much diminished as to affect not its relative, but its actual resisting power, and in the fifth, it was a little increased as well as the lower; to this circumstance is probably to be attributed the greater strength of this section than the fourth with a less inequality of the two flanches.

Since, in the last experiment, the casting broke by the separation of the extended side, it was probable that the strongest form was not yet attained. The experiments were therefore continued. But the general form or elevation of the beam was now altered.

The form before adopted, was that recommended by Mr. Tredgold as being of equal strength to sustain a load anywhere placed upon it, and therefore as being the most economical form. Since, however, it now appeared that the width of the lower flanch was a more important element in the strength than had before been imagined, since, moreover, the effect of the tensile power of this flanch would everywhere be greater as the resisting portion of the material in the upper flanch was more distant from it, it was clear that there would be an economy of the material, and, practically, a great convenience of form, in keeping the distance of the upper and lower flanch, throughout the whole length, the same, and varying the width of the lower flanch instead of the height of the beam, as had heretofore been done.

Under this new form the beam is represented by the two following diagrams, of which the first represents the *plan* of either flanch, and the lower the elevation of the rib which joins the two†.

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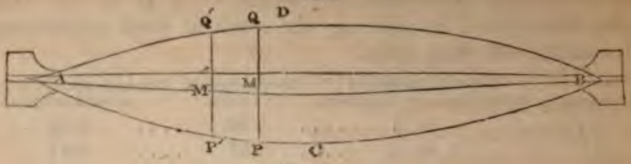
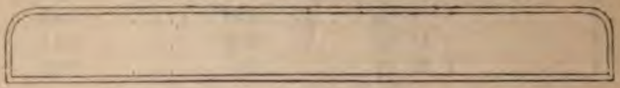
* The number of the experiment corresponds to the number of the diagram in the preceding page.

† *The curved form of each of the portions,* ACB *and* ADB, *of either flanch, was that of the curve called the parabola,* from the nature of which curve it follows that the widths, PQ, of the flanch at different points of its length, will be to one another as the products of the distances, AM and BM, from the two ends. Thus, for instance, the width, PQ, will be

Plan.



Elevation.

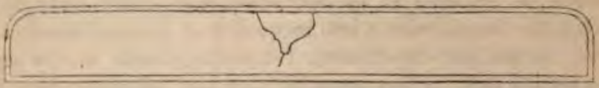


Having determined upon this general form of the beam, as involving a great economy of the material. Mr. Hodgkinson continued his experiments upon the best form of section, with beams thus constructed; the following table contains the result of them.

The depth of the beam, and distance of the points of support, was as before.

No. of Experiment.	Ratio of Sections of Compression & Extension.	Area of Whole Section. Inches.	Strength, per square Inch, of Section. lbs.
7	1 to 3·2	4·628	3246
8	1 to 4·3	5·86	3317
9	1 to 6·1	6·4	4075

In this last experiment, the casting broke by the compression of the upper flanch.



The above figure represents the form of the rupture. In every experiment up to this, the rupture had been by the yielding of the lower or extended portion of the beam; under this form of section, then, the material resists the forces tending to extend it, a very little more, and but a very little more, than those tending to compress it. Here then is the point at which it was the object of these experiments to arrive. The distribution of the material which gives an equal resistance to the forces of compression on the one side, and of extension on the other, is nearly that by which the lower flanch is made to contain about six times that of the upper. This, then, is the strongest form of section.

As this experiment, in point of fact, gave a greater strength per

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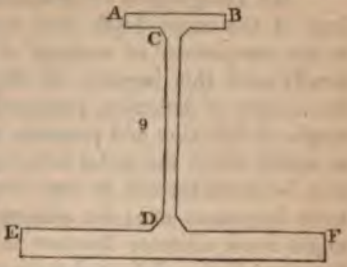
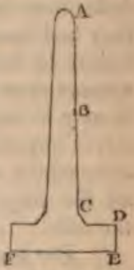
to the width P'Q', as the product AM × BM to AM' × BM'. And since the flanch is everywhere of the same thickness, its power at different points to resist the forces tending to extend or compress it there, will be as its widths at those points; this power will therefore be proportional to the products spoken of above; and since the distances of the flanches are everywhere the same, it follows that the strength of a beam, thus constructed, to resist rupture, *whether by* extension or compression, *will, at different* points of its length, be proportional to the products of the distances of those points from its extremities. Now, it is shown by all writers on the strength of materials, that the effect of the same force, applied at different points in the length of a beam to rupture it, is in this proportion of the product of the distances of the points from the extremities. The strength, then, of a beam thus constructed, is at different points in the same proportion as the effect of the force tending to rupture it. It is therefore throughout of the same strength.

inch of section than any other; it will be well to describe the section more accurately.

Its form was that of the following figure, but four times the size. The length of the upper flanch A B was 2·33 inches, and its depth ·31 inches, and in the lower flanch these dimensions were 6·67 inches, and ·66 inches respectively. The thickness of the vertical part, C D, connecting the two flanches was ·266 inches, and the weight of the beam was seventy-one pounds. Considerably more than two-thirds of the whole material of the beam was contained in the lower flanch. Now let us compare the strength of this beam with that of a beam of the form most approved before these experiments were made.



The girders, cast at the factory of Messrs. Fairbairn and Lillie, of Manchester, and most approved by them, were of the elliptical form (see diagram, page 196), and had the section shown in the annexed diagram. One of these was cast of the same length and depth as those in the preceding experiments, at the same time as the last, and of the same metal. Its dimensions were as follows:



Inches.
Thickness at A = ·30.
" " B = ·42.
" " C = ·45.
" " DE = ·51.
" " FE = 2·28.

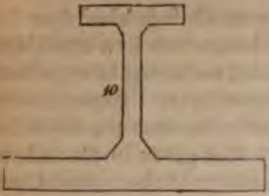
t broke with a weight of 9146 lbs.; and as the area of the section f fracture was 3·17 inches, the strength may be estimated at 2885 lbs. er square inch, and it may be stated, that of a number of experiments ith beams of this form, there was only one which gave a higher numecal strength, and that under peculiar circumstances. Now the strength er inch of section in Mr. Hodgkinson's improved form (Experiment 9), as 4075 lbs. *Here is a gain then in strength of the enormous amount of 190 lbs. on every square inch of section, by distributing the material of e beam according to that gentleman's form, being an increase of $\frac{2}{3}$ths. the whole strength of the beam.* And this, be it observed, over a am of the best construction before known—a construction greatly perior to that on which girders were then, and are indeed now ordirily cast. There is, however, another method of comparing the vantages of the two methods of construction, and that is by the ratio the strength to the weight of metal.

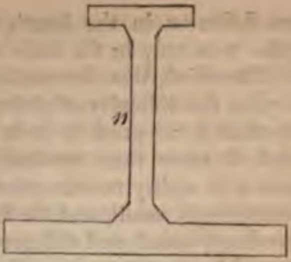
The casting of the best construction (Experiment 9), broke with a eight of 26,084 lbs., and the casting weighed 71 lbs., each pound in eight gave therefore 367·38 lbs. in strength. The casting on Messrs. *irbairn and Lillie's original* construction of the same length and depth, *ighed 40 lbs., and it broke* with 9146 lbs., so that each pound in the weight of this casting corresponded to 228·65 lbs. in strength. *The strength given by each pound weight of metal in this beam was, therefore, 138·73 lbs. less than that given by each pound of the other.*

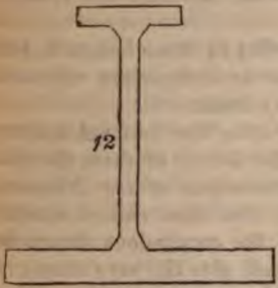
Mr. Hodgkinson's experiments had hitherto been confined to castings of the same depth and length. His inquiries were now directed to the comparison of castings of different depths and lengths. Collaterally with this inquiry, he directed his attention more particularly to the *amount* of deflexion, produced by a given load, and to the different stages of deflexion and pressure, under which the elasticity is destroyed, or under which the metal is technically said to take a set. This inquiry may be characterized as one into the stiffness of the casting. It is a most important one; for although beams, cast on the new principle, might resist ultimate fracture more successfully than those of the old form, yet if they deflected more under a given pressure, or if under such deflections they more readily fixed themselves in those forms into which they had been deflected, partaking thus in a degree of a quality analogous to flexibility, there might result from these causes inconveniences more than counterbalancing the increase of ultimate strength. It may be mentioned in the outset, that these suppositions were in themselves improbable, and anomalous; it was to be expected that the difficulty which attended the ultimate fracture would in a degree characterize all the stages of approach to it, and such in reality was found to be the case.

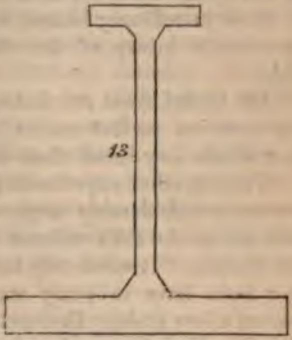
The beams were now cast 7 feet 6 inches long, and the props placed 7 feet asunder. The ratio of the upper and lower flanches was, in each experiment, that of 1 to 6, which had been ascertained to belong to the best form of section, and they were all of the same size, being, indeed, cast from the same model, which was only varied by increasing in each experiment the *distance* of the flanches, or the *depth* of the rib which joined them. The following diagrams represent the sections of fracture, each as before one-fourth its real size.

In the first of these experiments (diagram 10), the depth of the beam was 4·1 inches. The beam was loaded with 2764 lbs., when the deflection was 0·25 parts of an inch, and on the removal of this load it returned to its original form, showing that the elasticity had not been strained. The load was then gradually increased up to 3339 lbs., and the deflexion to 0·28 inches, and still the beam recovered its form; 3454 lbs. were then put upon it, and there was now a perceptible set, or permanent deflexion, but of exceeding small amount. With a load of 3914, this permanent deflexion became 0·05 inches. The load was then further increased until it became 6215 lbs., and the deflexion 0.51, and throughout the whole of this increase no further set was apparent, the beam returning, when the load was removed, always to its first permanent deflexion of 0·05. When, however, the load was made 6971 lbs., a new set became apparent, and when it was increased to 8637 lbs., this set was measured to 0·03 more than the first, or the whole permanent deflexion was now made 0·08 with a load of 11,397 lbs., this permanent deflection became 0·09, with each further increase of weight; the perma*nent deflexion* began now rapidly to increase, until with 12,815 lbs. it *became 0·14*, and with 13,543 lbs. the beam broke.









Similar circumstances characterized the other experiments. In the second (see diagram 11), the depth of the beam was 5·2 inches—the first perceptible set took place under a load of 7257 lbs., with 7947 lbs. it was 0·08, and the deflexion 0·35. It bore 12,087 lbs. with a deflexion of 0·63, and broke by extension with a weight of 15,129 lbs. In the third experiment (see diagram 12), the depth of the beam was 6·0 inches, and the first perceptible set took place under a weight of 13,543 lbs., and a deflexion of 0·49 inches. The beam broke with 15,129 lbs. In the fourth experiment (diagram 13), the depth of the beam was 6·93 inches, and the first perceptible set took place with a load of 14,271 lbs., and a deflexion of 0·35. The beam broke with 22,185 lbs. These results may be tabulated as follows:—

No. of Experiment.	Depth of Beam in Inches.	Load under which a Set is first taken in lbs.	Deflexion under which a Set is first taken in Inches.	Greatest Deflexion before Fracture, in Inches.	Breaking Load, in lbs.	
1	4·1	3454	·28	1·08	13543	
2	5·2	7257	·35	·63	15129	This Beam was weakened by a twist in the vertical part of the casting.
3	6·0	13543	·49	·58	15129	
4	6·93	14271	·35	·65	22185	

From these experiments, the first conclusion to be drawn is, that other things being the same, the ultimate strength is, in beams of this form, nearly as the depth, but in a somewhat lower ratio.

The second, that the stiffness of the beam rapidly increases with its *depth, a weight nearly twice* as great being required to produce the same deflexion in the fourth experiment as in the first, although the depths were only in the ratio of 3 to 4.

The third, that the quality of elasticity as distinguished from flexibility, or the difficulty of giving a set to the beams was in a much higher ratio than is assigned to it in ordinary beams, requiring in experiments 2 and 4, more than one-half the breaking-weight. Experiment (1) presents in reality no exception to this remark, because the depth there is exceedingly small, and the first set which is that given in the table was exceedingly small, and did not affect the elasticity of the material, being followed by no other set until the weight was nearly doubled.

Now in ordinary beams, it appears (Tredgold, page 79), that there is a sensible injury of the elastic force with one-third the breaking-weight.

Mr. Hodgkinson concludes, therefore, that in these beams the elasticity remains perfect under loads greater in comparison with those under which they break than in the ordinary beams.

Various other experiments were made after the first publication of these on a considerably larger scale; for the details of them the reader is referred to the fifth volume of the *Transactions of the Manchester Phil. Society*. It need only here be stated, that these experiments on a larger scale have in every way supported the conclusions drawn from those on a less scale. In conclusion, we shall give the very simple rule which Mr. Hodgkinson has deduced from these experiments to estimate the strength of beams cast on his construction.

RULE.

If A be the area of a section of the bottom rib in the middle of the beam, and if D be the depth of the beam there, and L the length or distance between the points of support, all these dimensions being in inches, then will the ultimate strength of the beam in tons, when it is cast erect, be represented by the formula,

$$\frac{26 \times A \times D}{L}$$

And when it is cast on its side by

$$\frac{25 \times A \times D}{L}$$

That is, if taking the dimensions in inches, we multiply the area of the middle section of the lower rib by the depth of the beam, and divide this product by the length of the beam, then 26 times this product will represent the number of tons' weight which will just break the beam when it is cast upright, and 25 times the product when it is cast on its side.

It is manifest that the principles which Mr. Hodgkinson has established for the best sections of girders, are applicable, with the proper modifications, to every form under which cast-iron is employed to sustain a transverse strain of the material.

INSTANTANEOUS LIGHTS.

The tinder-box has been employed from time immemorial for the purpose of obtaining a light: it is now, however, nearly expelled from our dwellings by a host of ingenious chemical contrivances, called "Instantaneous Lights." A popular account of some of these may, perhaps, be acceptable to the readers of this Magazine.

About the year 1673, *phosphorus* was discovered, and its singular inflammability was taken advantage of, as a refined chemical method of getting a light quickly. A small portion of phosphorus, rubbed between the folds of brown paper, instantly bursts into a flame, which will kindle a common brimstone match. This method was eagerly adopted by all such persons as could procure so great a chemical curiosity; for phosphorus was by no means plentiful until the year 1680, when Godfrey Hanckwitz manufactured and sold it in large quantities at his laboratory, in Southampton-street, Strand. [This laboratory, and much of its curious apparatus, is yet in existence.] The marvellous properties of phosphorus excited much attention in this country, and Godfrey set out on his travels abroad, to exhibit and vend the article; he, therefore, has the merit of generally introducing the first chemical method of getting a light.

Improvements were, of course, soon made upon it; one of which was to coat the wicks of small wax-tapers with phosphorus, in glass tubes, hermetically sealed; thus constituting what were called "phosphoric tapers." When a light was required, one end of the tube was cut off with a file, and the taper being quickly drawn out, it took fire upon touching the air; but this plan, although ingenious, was not so simple as the original method of friction; it was inconvenient, sometimes even dangerous, and never came into general use. It was succeeded by placing a bit of phosphorus in a small phial, and then stirring it about with a hot iron-wire, thus partially burning it in a confined portion of air, and covering the interior of the phial with oxide of phosphorus: it was then corked up tightly after removing the wire, and preserved for use. When a light was wanted, a common brimstone-match was put into the bottle, and a small portion of the phosphoric compound withdrawn upon its brimstone tip; flame instantly resulted, from the strong affinity of the sulphur for the phosphorus. This method, from its simplicity and durability, had a long run, and may even now be met with. Another plan with phosphorus was, to take a small bit of it on the point of a brimstone-match, and then to rub it on a cork or piece of soft wood; the friction caused the union of the phosphorus and sulphur, with the evolution of flame.

These methods with phosphorus were succeeded by a substance called *pyrophorus:* it was a black powder, produced by the calcination of flour, sugar, and alum, and having the singular property of taking fire upon mere exposure to air. A small bottle of pyrophorus, well prepared, lasted a considerable time, and was a good method of getting a light. The theory of its action was long a mystery; but we now know, that the fire of the pyrophorus, or *fire-bearer*, results from the attraction of oxygen for potassium, which inflammable metal is elicited from the potash of the alum by the action of the charcoal of the flour and sugar. "Homberg's pyrophorus," for so it was named, had a long day, but chiefly with scientific curiosos. The invention which first bore the name of "Instantaneous Light Machine," was the "inflammable air-lamp of Volta;" an extremely elegant and scientific apparatus, consisting of a glass reservoir filled with hydrogen-gas (or inflammable air, as it was then called), which could be subjected to the pressure of a column of water upon turning a stop-cock. The pedestal upon which the reservoir was placed, contained an electrophorus (a variety of the electrical machine), the apparatus being so adjusted by connecting wires, that upon turning the cock a small stream of hydrogen rushed out, and met with a spark of electric fire, which caused its combustion; and this flame kindled a wax-taper, placed directly against it. This machine was soon modified into a variety of ornamental forms, and quickly found a place in the study of almost every scientific man. One fatal objection to its general introduction was, its tendency to explode,—a most disagreeable tendency certainly, especially if the light-seeker was in a great hurry to seal a letter, to say nothing of the squirting of the acid-water over papers, books, and furniture, and perchance the fracture of a looking-glass or window, by a flying fragment of the gas-reservoir. Such an accident often happened, and the Light Machine was denounced as an "Infernal Machine."

The researches concerning the evolution of heat, by the compression of air, led to the introduction of its agency for obtaining a light.

A small stout brass tube, about six inches long, and half an inch in diameter, closed at one end, and fitted with a hollow air-tight piston, containing in its cavity a scrap of *amadou* or *German tinder*, constituted the apparatus which was called, "The Pneumatic Tinder-box, or Light Syringe," and which was used as follows:—The piston was suddenly driven into the tube by a strong jerk of the hands; the air in the tube thus compressed, had its capacity for heat diminished, and therefore parted with it in sufficient quantity to cause the ignition of the tinder; and upon quickly drawing out the piston, the glowing tinder would, of course, kindle a match*.

The Light Syringe is even now frequently used on the continent, although we very seldom see it here, unless upon the lecture-table of the chemist. Considerable practice is required to learn the right method of using it, and a novice has to undergo sundry abrasions of the knuckles, fingers, sprains of the hands, &c., before he lights upon the proper knack of suddenly and successfully compressing the air. The agency of Voltaic electricity was next pressed into the "light company;" it was found that a plate of zinc, and a double plate of copper, when dipped into a dilute acid, evolved sufficient electricity to ignite a fine platina-wire connecting them. This apparatus, being very simple and scientific, was adopted by many, but more especially by philosophers. It took up very little room, as a single pair of plates, two inches square, and a little cistern of acid, about the size of a snuff-box, was quite adequate to the ignition of a fine filament of platina-wire, at which a bit of touch-paper could be ignited, and then a match from this. It would not answer to apply the match directly to the ignited wire, as the sulphur combined with the platina. All the contrivances which have been now spoken of, quickly vanished into the shades, before the sudden blaze of "The Oxymuriate Matches." It was discovered that the singular salt called oxymuriate, or chlorate of potash, when mixed with sugar or other inflammable matters, caused them to enter into sudden combustion upon the contact of a drop of vitriol.

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* *This apparatus*, somewhat modified, was employed by the French as a *substitute for the gun-lock* on fire-arms, before the introduction of percussion-caps.

Small portions of such mixture in powder were first employed, but it was soon mixed with gum-water into a paste, (together with a little vermilion, for the sake of colour), and with this thin slips of deal were tipped, to constitute oxymuriate matches, which were put into a neat little case of crystallized tin, or *moirée métallique* (for that beautiful manufacture was just come into fashion), with a bottle containing a bit of asbestos soaked in strong oil of vitriol: cases thus fitted up were improperly, but almost universally, called "Phosphorus-boxes." The addition of a wax-taper was made soon afterwards, and the matches, bottles, and boxes, appeared in swarms, of all colours and dimensions. Competition at last became so great, that from fifteen, ten, and five shillings, the price fell to half-a-crown for a small box, and this price continued steady for some years.

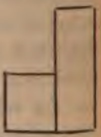
The theory of the action of these "phosphorus or instantaneous light-boxes," consisted in the sudden decomposition of the chlorate paste by the oil of vitriol, and the evolution of oxygen, which entering into energetic combination with the inflammable elements of the sugar, produced flame. It was a very complex action, and understood by none but chemists; but every one could appreciate its practical application, and nothing more was wanted.

Sometimes this production of fire took place so instantaneously that the wood of the match had not sufficient notice to light; at others, in consequence of the weakness of the acid, or badness of the composition, not a spark of fire was evolved, but the red tip merely fizzed, and spurtled the vitriol over ladies' silk-dresses, to their utter spoilation. By the way, about fifteen years ago, when this invention started, ladies were not so "darkly, deeply, beautifully blue," in chemical matters, as they are now-a-days; and very frequently, when the match did not light instantaneously, they innocently returned it to the case amidst the others, and selected a fresh one: the rejected match still retaining a spot of vitriol, imparted it to the tip of its next neighbour, this being perchance a good one, entered into rapid combustion, which instantly spread throughout the whole legion of fiery spirits, and out they all shot from the box, like a small girandole flight of sky-rockets, to the great terror and discomfiture of the female experimenter. This firework happened when the tips of the matches were placed *downwards;* and complaints of the danger being made to the match-maker, he, to avert it, placed the tips *upwards;* but this plan was even worse than the other, for if great care was not taken in withdrawing a match from the vitriol-bottle (and *especially in the dark), it might* touch the combustible tips of the others *and set them blazing also;* but they would not be projected from the box, as in the downward plan. However, both these inconveniencies were soon remedied, by making a little tin cover to the match department, and giving printed directions to shut the cover down upon taking out a match; and if this did not light, to throw it away, and never return it to the case.

The chlorate paste was gradually improved, and the matches still more perfected by tipping them, first with sulphur, and then with the paste: thus they lighted with more certainty; frankincense and camphor were sometimes mixed with the composition, and the wood of the match was pencil-cedar, so that a fragrant odour might be diffused during the combustion. It was soon found that the cork of the bottle became corroded and rotten, and the vitriol weakened by exposure to the air. A ground-glass stopper was therefore substituted for the cork, and this again covered with a ground-glass cap, still more effectually to exclude the air: some persons preferred touching the match with the vitriol, instead of the vitriol with the match, and for their accommodation an elongated stopper was provided, which would withdraw a drop of vitriol. Those who did not like the expense of caps and stoppers, were provided with a bottle, having a plug of India-rubber, instead of a stopper or cork, for that substance was not acted upon by vitriol*.

But although the instantaneous-light box was now tolerably perfect and certain in its operation, half-a-crown for a plain box, and a shilling per hundred matches, was a high price, and beyond the reach of every individual. It rapidly fell by the appearance of a placard, with the following announcement:—"Save your knuckles, time, and trouble: use Heurtner's EUPYRION, price one shilling!" and accordingly the Eupyrion found its way, like magic, into the parlour, bed-room, and kitchen. The housemaid, whose chapped knuckles had many a time and oft received the gashing stroke of the flint, when striking a light on a dark frosty morning, was loud in the praises of *Hewperryon*. In the study or parlour, a letter or *billet-doux* could be sealed and sent off quietly, without "ringing for a light;" and in the bed-room a light could be obtained in case of illness or sudden emergency. Almost everybody was possessed of the Eupyrion; it even found a place on the shelves of the most celebrated laboratory in this country, and facilitated many of the operations of the "philosopher by fire."

This invention consisted of two tin cases, about one inch and a quarter square; but the one three inches, and the other about an inch long, they were soldered together thus,—the shorter one holding the vitriol-bottle, the other the matches with their tips upwards; by this simple arrangement there was very little chance of any accidental combustion occurring, as the bottle and matches were not on a level; the composition was good, the vitriol strong, and rarely failed to produce an instantaneous light; the cork did not corrode, partly because it was covered with thin lead, and partly because there was no superfluous



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* The bottles of the Instantaneous Lights belonging to the thermometer for measuring *accessible* heights, were always fitted with plugs of India-rubber, although this plan has been claimed as a more recent invention.

vitriol in the bottle. The matches soon fell in price to four-pence per hundred, then three-pence, two-pence, and now they can be bought for one penny per hundred*.

Dependent upon chlorate matches and vitriol, but with the addition of a spirit-lamp and machinery, appeared "the day or night self-illuminating lamp," bearing, in letters of gold, the imposing motto, *In Hoc lumine vinces*. It was rather a complicated affair, but may be thus described. To the elongated stopper of the vitriol-bottle was attached a string, passing over a small pulley in a brass standard, fixed to the box, and this string was carried (like the line of a bird-catcher's trap,) to the bed-side of the operator. Upon being pulled, the stopper rose from the bottle, carrying with it a drop of vitriol, and the same movement caused a spring to press a match in contact with it; flame resulted, which was caught by the wick of the spirit-lamp; but as the flame of spirit of wine is by no means luminous, the operator had to get out of bed after all his fire-catching, and put the wick of a common candle to the spirit-lamp, in order to illuminate his dormitory. The idea, although ingenious, hardly warranted the assumption of the travestied motto, *in hoc lumine vinces;* and the invention never came into general use; its price also was very high.

About this period many improvements were made in the instantaneous-light boxes, which it is impossible here to enumerate, but the main principle remained unchanged. The discovery of the singular property of spongiform platina igniting by the contact of a jet of hydrogen, gave rise to its introduction as an instantaneous light-machine, and the apparatus made its first appearance in a form closely resembling that of the Volta's machine already described, excepting that no electrophorus was required. A small cup, holding a bit of spongy platina about the size of a pea, was opposed to the jet of gas from the stopcock; ignition resulted from this contact, the hydrogen inflamed, and readily lighted a candle or lamp. This invention underwent a great variety of complications and simplifications, but the principle remained unaltered, and it is employed even at present, but it is quite as apt to explode as the inflammable air-lamp, and even more subject to get out of order. No satisfactory theory of its action has been offered, and all we know about it is, that when extremes meet, light results; that is, when the *heaviest* body, platina, comes into contact with the *lightest*, hydrogen, ignition takes place; this discovery is due to Döbereiner.

But now, in the year 1836, all the instantaneous lights, and light-machines, are fast retreating before the powerful host of "Lucifers and Prometheans."

LUCIFERS consist of chlorate of potash mixed with sulphuret of antimony into a paste with starch, the ends of chips of wood are tipped with this composition, which inflames upon the friction of glass-paper.

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* Such is the competition at present in the instantaneous-light trade, that one dozen of complete "*fire-boxes*" may be bought for one shilling.

This is a very ready means of getting a light; but the sulphureous antimonial vapour emitted during the combustion, is by no means pleasant, especially to persons of weak lungs.

PROMETHEANS are a clever refinement upon the oxymuriate matches, consisting of a small roll of waxed paper, in one end of which is a minute portion of vitriol in a glass bulb, hermetically sealed, and surrounded with chlorate paste. When the end thus prepared is pressed so as to break the bulb, the vitriol comes in contact with the composition, and produces an instantaneous light; just reversing the old operation of putting the match into the bottle, we in this instance find the bottle (the bulb) put into the match. This invention may be considered as the *ne plus ultra* of chlorate matches; but prometheans and lucifers must be used with caution, and should never be carelessly left about. For the accommodation of cigar-smokers, prometheans are made with touch-paper; this ignites from the composition, and glows without flame, like a slow-match, and as the wind will not extinguish it, a good cigar may be readily lighted. One word of caution to stage-coach travellers who use this invention. Take care when the burning match is dropped, or otherwise the coach may be set on fire; never throw away the ignited remains of a match or cigar, in the vicinity of villages or farm-yards, or perchance, an incendiary fire may unwittingly result.

If the elements of a promethean were inserted into the end of a cigar, it would light upon mere pressure, and would also be attended with the advantage of informing novices which was the proper end to light; many young sparks for want of knowing this, and being diffident of asking, very frequently light the wrong end, which is a sad inconvenience*. Such then is a short history of the rise, progress, and present state of the leading varieties of instantaneous lights.

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* Some contrivance of this kind has been adopted by our transatlantic neighbours since this paper was written.

PHILOSOPHICAL TRANSACTIONS, 1836, PART I.

OUR Journal being devoted to "Popular Science," does not imply that we pass over physical inquiries on account of their abstruseness. Our object is, on the contrary, to convey, in language as intelligible as possible to general readers, the facts which have been observed,—the laws which have been deduced from them,—and the methods which have been employed, in the different researches of philosophers for the extension of science, in its proper sense of the term. With this object in view, we have given analyses of several memoirs of the first importance on different branches of physical science in our former numbers, (as, for instance, the *tides*, *light*, and *terrestrial magnetism;*) and we purpose to continue this plan as circumstances shall enable us, till we have laid before our readers a succinct account of the present state of every branch of physical science.

There are, however, many subjects of a more isolated character, and upon which very little is actually known calculated to interest others than the few who are engaged in those special inquiries. These are for the most part published in the transactions of scientific societies. On that account, we think it will best meet the views of all parties, to take those transactions when they appear, and give of the several papers more or less extended accounts, as their interest seems to demand; and occasionally interspersing a few collateral remarks on the state and nature of the inquiry generally. Our readers will then be often able to ascertain whether any particular dissertation is likely to be of interest to themselves, and often perhaps be led to consult a memoir, which otherwise they might have passed over in ignorance of its existence, or in dread of encountering the supposed difficulties of its *mode of treatment* in such memoirs.

With this view, we commence with Part I. of the *Philosophical Transactions*, just published.

I. *On the Empirical Laws of the Tides in the port of Liverpool. By the Rev. W. Whewell, M.A., F.R.S.*

VIII. *Discussion of the Tide Observations made at Liverpool. By J. W. Lubbock, Esq. F.R.S.*

XII. *On the Solar Inequality, and on the Diurnal Inequality of the Tides at Liverpool. By the Rev. William Whewell, F.R.S., Fellow of Trinity College, Cambridge.*

We have already, in the main, given the results of these researches in a former number; and shall at a future time resume the subject, embodying therewith some other investigations relative to it.

III. *An Account of the Great Earthquake experienced in Chile on the 20th of February*, 1835. *By Alexander Caldcleugh, Esq., F.R.S.*

IV. *Some Account of the Volcanic Eruption of Cosegüina in the Bay of Fonseca, commonly called the Bay of Conchagua, on the western coast of Central America. By the same.*

We shall *publish in our next* number these two interesting papers entire, *and hence abstain from any* analysis of them here.

V. *Memoranda made during the appearance of the Aurora Borealis, on the 18th of November, 1835. By Charles C. Christie, Esq.*

These observations were made from the drawing-room window of Deal Castle, and from the top of the building. They are, perhaps, the most interesting that have ever been recorded, inasmuch as they set at complete rest the question respecting the *region* of the Aurora. They are illustrated by four lithographic views of its appearance in different stages, without seeing which any description must fall far short of conveying an adequate idea of the phenomenon,—though even these fall far short of the drawings themselves, which we had an opportunity of seeing on the night the paper was read. The subject deserves the attention of every one who is interested in inquiries respecting this yet unexplained phenomenon. We annex Mr. Christie's concluding remarks, which we are assured must be interesting to every one.

"Having at the time no instruments for determining the bearing or altitude of the arch, I was obliged to depend upon the positions of some conspicuous stars, which were conveniently situated for that purpose. According to these rough data, the altitude was 18°; the angle subtended by the span of the arch, about 130°; the bearing of the centre of the arch north-north-west, true, or very nearly magnetic north; and the arch was consequently at right angles to the magnetic meridian.

"The body of light was nearly colourless; its brightness was similar to that seen on the edge of a cloud when the moon is about to rise behind it, with, however, this striking difference, that the stars were distinctly seen through the diffused light of its upper surface, and those in the tail of the Bear shone clearly in the very body of the light on the right hand.

"With regard to the sketches with which I have attempted to illustrate the preceding notes, it is necessary to observe, first, that the extent of horizon renders it impossible to give, in one view, any idea of the magnificent scale on which the original was depicted, or even to preserve very correctly the relative proportions of height and breadth; and next, that in sketches I. and II., the rapid motions of the bodies of vapoury light, and of the flame-like pencils, must be held in mind: the former bore an exact resemblance to the faint reflected light darting across the sides of a room from a mirror turned sharply in the hand, and the latter to the lambent flames which diluted spirit of wine, poured on a flat surface and ignited, will exhibit when half extinguished.

"The pencils which appeared in front of the dark cloud, of which there were not more than three, were very distinct in their character, from the others; they were of a yellower tinge, and extremely narrow throughout their whole height. I have stated, that they *issued from* the dark cloud; perhaps it would be more correct to say, that they *pierced through* it; for although I did not observe the instant of their appearance, being at the moment engrossed by the display on the left, yet, in each, the brightness of the base, which was, as it were, the nucleus of its light, seemed to warrant this idea. The mere circumstance, however, of their appearance in front of the cloud, tends to elucidate a point on which there exists much difference of opinion, the height of the aurora in the atmosphere. The dark cloud itself can scarcely be supposed to have occupied a very elevated region; and it is *manifest, that if* these brilliant pencils had their origin *in*, or *in advance of*, *the cloud, their* bases must have been of inferior altitude to its upper portion, nd equally so, if they were identical with any continuation of the luminous latter of the arch, concealed by the cloud.

"The first appearance of the aurora at nine o'clock, was that of a dark nvex cloud, cutting off the luminous arch, and concealing a body of light ehind, the eye naturally referring the light to a more distant region, while e sharp line of division threw the cloud forward. Subsequent appearances, wever, did not seem to confirm this notion, but, on the contrary, induced e to consider, whether the dark cloud might not be a *substratum* of matter iffering in nature and density from the superincumbent arch of light. The llowing are the facts which appear to favour this supposition. First; every reat outbreak of coruscations from the luminous arch produced a correspondg disturbance in the part of the cloud immediately below. Thus, during e display at 9^h 15^m, the arch was gradually losing its regular form on the ight, and at 9^h 20^m I noted it 'very irregular, with a large indentation on he eastern side,' while on the west, where the body of light was undisturbed, he arch remained perfect. Thus also, immediately after the western half ad been in vivid coruscation, the whole of the arch was 'dilapidated,' and, nally, 'entirely broken up.' Secondly; neither the straight nor waving encils appeared to proceed from behind the cloud, but always from the pper surface of the light. Thirdly; when the arch was 'dilapidated,' it was ot merely its upper surface which was of irregular form, but masses of it ere lying in confusion, separated from each other by a boundary of light, ot appearing in the least as if light behind were shining through, but rather s if the substances of the arch and cloud had unwillingly interpenetrated ach other, and refused to mix together more intimately, while at the same ime the light above became more diffused, and of diminished brightness. astly; when at 9^h 35^m the continuity of the arch is restored, it remains of irregular undulating form,' while fainter pencils continue to rise from very part; at 9^h 55^m the cloud is 'much darker,' 'the pencillings very faint,' hile its form is evidently becoming more regular; and at 10^h 20^m it is early perfect in form, 'strongly defined and steady,' the pencillings having eased.

"All these circumstances struck me as so closely resembling the disturbance of two fluids, the one superposed on the other, mutually repulsive, but compelled to mingle by forces, of whose action the vividness of the pencillings seemed to indicate the intensity, and requiring intervals of repose to recollect their scattered energies, that I cannot but conclude the luminous matter of an aurora to be a superincumbent stratum, and, consequently, that its altitude is dependent on that of the dark mass immediately beneath."

VI. *On the Anatomical and Optical Structure of the Crystalline Lens of Animals. By Sir David Brewster, K.H. L.L.D. F.R.S.*

This important paper contains the results of the examination of the uperficial structure and optical properties of the lenses of a great numer of animals and fishes; and is continuous of a paper in the *Transctions* for 1833, by the same author, in which that of cod is particularly xamined, and compared with those of a considerable number of birds nd fishes, and also with two of the lizards.

The general form, geometrically considered, is that of a surface of evolution, *the axis of revolution* coinciding with the axis of vision. *here is only one exception to this*—the elephant, whose eye approaches

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This structure appears to be universal in birds, and the most common one in fishes. In the mammalia and cetacea it has not been discovered at all; and in many kinds of fishes a different one is found to exist.

In fishes, the figure of the eye generally approaches to spherical; but in few of them are the two axes quite equal. If, now, we suppose a short horizontal line to be traced upon the anterior face of the lens, and an equal vertical one on the posterior face, each passing through the pole of the face on which it is traced; then the fibres emanate from different points of these lines, and pass round the equator of the lens, till they terminate in a corresponding point of the other line diametrically opposite to its origin. These fibres, except those which emanate from the extremities of the *septa*, or lines, are not wholly in the same plane, but pursue a winding or spiral course. They are curves of double curvature*; which yet deviate but little from the plane to which they are symmetrically related.

This structure he found to prevail in twenty-six fishes, three quadrupeds, and five reptiles.

He then proceeds to describe the more complex structures of the lenses of the mammalia. Except the hare, the rabbit, and the *Perameles nasuta*, whose structures are similar to those of fishes, the simplest of these is—three lines, or septa, radiating from each pole, forming angles of 120° with each other; whilst, if all the septa were orthographically projected on the equator of the lens, the projections of the posterior septa would bisect the angles formed by those of the anterior ones, and *vice versâ;* they would resemble lines drawn from the centre of a regular hexagon to its several angles. From these the fibres radiate by a more *bizarre* course, and finally terminate in the symmetrical point of the opposite septum. The serrated structure of these fibres was generally discernible. The figure of the posterior and anterior surfaces was invariably found to differ in convexity. Forty-four animals were found in which this structure prevailed: and one anomalous case was observed of a fish, the name of which was unknown, caught near the Azores.

A fourth variety was observed in most of the cetacea examined. The septa were here four, forming on each surface a rectangular cross; the projections of the two crosses, as in the last case, bisecting each other's angles.

A fifth is, where the second structure terminates in two branches at right angles to each other, and making equal angles with the original septa; and a sixth, where the third structure so terminates: the fibres, in all cases, terminating in symmetrical points of the opposite septa.

Sir David also examined these lenses, with respect to their several influences upon polarized light. These do not appear to show the same kind of regularity that the structure might at first sight lead us to expect. For instance, the hare depolarized *two* series of luminous sectors, the

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* Sir David Brewster, for what reason we know not, has called these figures "curves of contrary flexure." This term had already been appropriated to designate a character of *plane curves,* whilst the *other had been as regularly employed to express curves not lying in one plane.* We should not have alluded to so inconsiderable an oversight (for such it most likely is), except to warn his readers of the error of his phraseology, and prevent such a perversion of the use of terms already and universally appropriated to distinct and specific purposes.

inner sectors having the *negative structure, like calcareous spar*, and the *outer sectors* the *positive structure, like zircon;* whilst the depolarized sectors given by the rabbit were *wholly negative.* In the salmon, whose fibres have the same anatomical structure, the eye depolarized *three sectors of light, the inner and outer series being negative, and the inner positive.*

VII. *On an Artificial Substance, resembling Shell. By Leonard Horner, Esq., F.R.S. L. and E., with an examination of the same. By Sir David Brewster, L.L.D., F.R.S.*

On going through the bleaching-grounds of Messrs. Finlay and Co., of Catrine, some time ago, Mr. Horner was struck with the unusual appearance of a part of the machinery, which at a distance appeared to be of brass; but on a closer inspection he found it to be a large circular wooden box, coated with an incrustation of a brown compact substance, having a highly-polished surface, a metallic lustre in some places, beautifully iridescent, and when broken, exhibiting a foliated texture.

The part of the machinery on which he observed this deposit, is a large circular box (called the Dash-wheel) revolving on a horizontal axis. Its purpose is to rinse the cloth in pure water after it has been boiled and steeped in the bleaching-liquors. It turns at the rate of twenty-two revolutions in a minute.

The uses to which it was applied obviously intimated the source where to search for the origin of the deposited substance. The information he received was, that it was an incrustation of carbonate of lime; and he very naturally inquired, Whence the brown colour and the metallic nacreous lustre? If the substance was, as he conjectured, analogous to shell, whence could the animal matter be derived? He thus traces its probable history.

> "The cotton-cloth is brought to the bleach-field in the state in which it is taken from the weaver's loom. The first process is to steep it in water for several hours, after which it is immersed in cream of lime. This is made in the following manner; fresh-burned lime is slaked and passed through a fine sieve, and added to water in the proportion of 38 lbs. of dry lime to 1000 lbs. of cloth. The cloth is boiled in this liquor from four to six hours, the lime acting as an alkali; and it is used only from being considerably cheaper than potash or soda. After this boiling, the cloth is taken to the dash-wheel to be thoroughly cleared of the lime, which is effected by its being tossed about for ten minutes in clear water in the interior of one of the compartments into which the wheel is divided. Here, then, is the source of the calcareous matter of the incrustation; and we have the lime dissolved or suspended in the water in a state of extremely minute division, and from which it is deposited, most probably, by a partial evaporation. It is difficult to say whether the deposit takes place while the wheel is revolving, by the water being broken into a kind of spray, and so presenting a greater surface for evaporation, or during the night, when the wheel is still: some of the properties, to be afterwards described, render the latter supposition the most probable. But in whatever way it takes place, the operation is an exceedingly gradual one; for the wheel had been in constant use for ten years, and the coating in the interior did not exceed one-tenth of an inch in thickness. It had been in our [illegible] about two years before any perceptible deposit showed itself in the

inside; but it had not been going half a year before an incrustation began to be formed on the outside of the wheel. I remarked that the deposit was in greatest quantity around the orifice where the cloth is put in and taken out. The deposit in the interior, and which coated the whole surface of the compartment, was of a darker brown colour, and was as smooth and splendent as a lining of highly-polished bronze would have been. The high polish is no doubt partly produced by friction; and I observed that it was highest on that part of the outside nearest the opening.

"So far we have *calcareous*, but no *animal*, matter; but in going a little further back in the history of the process to which the cotton had been subjected, before it came to the bleach-field, I discovered that animal matter might be contained in the incrustation. I learned that the cloth had been woven in power-looms; and on making inquiry as to the composition of the dressing or paste used to smooth and stiffen the warp before it is put into the loom, I was told that in the factory from whence the cloth had come, it is the practice to mix glue with the wheaten flour, generally in equal proportions by weight.

"We have thus lime and gelatine, the same materials which are employed by the molluscous animal in the formation of its covering, and apparently in the same degree of minute division as that in which they are exuded from its mantle."

He then gives the chemical examination of the substance, and shows very clearly, that this substance is formed by a process very analogous to that by which the shells of the mollusca are generated.

Its optical properties are then exhibited in a letter from Sir David Brewster. In point of hardness it lies between calcareous spar and Oriental pearl on one side, and mother-of-pearl and oyster-shell on the other. He considers that in its polarizing structure this new substance has the same relation to calcareous spar, that mother-of-pearl has to arragonite.

When we look through the substance perpendicularly to the surface, the candle appears surrounded by a halo of nebulous blaze; but if it be ever so little inclined to the axis of vision, in any azimuth, the light is separated into three distinct *parallel crescents* (like the moon when two or three days old), having their concavity towards the end of the plate nearest the eye. In the middle one, only the flame of the candle appears. The inner and middle crescent are polarized in a plane passing through the direction of their length; whilst the outer one, and the image of the candle itself, are polarized in the opposite plane.

Sir David then gives the rationale of these phenomena, which to us appears perfectly satisfactory; and he deduces some conclusions of importance in respect to the colours of the different pearly substances.

IX. *Geometrical Investigations concerning the Phenomena of Terrestrial Magnetism. Second Series:—On the number of Points at which a Magnetic Needle can take a position vertical to the Earth's Surface. By Thomas Stephens Davies, Esq., F.R.S. L. & E. Royal Military Academy.*

We have already given an analysis of this paper in the *Horæ Magneticæ in our sixth number.*

X. *On Voltaic Combinations. In a Letter to Michael Faraday,* D.C.L., *F.R.S. By J. Frederic Daniell, Esq., F.R.S. Professor of Chemistry in King's College, London.*

XI. *Additional Observations on Voltaic Combinations, in a letter from the same to the same.*

The object of these papers is to describe a method of constructing a battery, whose action should be *uniform*—an object of paramount importance in all researches concerning the *laws* of voltaic action. In accomplishing this, Professor Daniell has been altogether successful; and we trust the apparatus thus put into our hands will soon lead to some definite results, from which a mathematical theory will be ultimately educed. The papers do not admit of abridgment, nor of an effective analysis, without employing figures; and, indeed, all whom it, from their studies, is calculated to interest, will make a point of reading it for themselves.

XIII. *On the Action of Light upon Plants, and of Plants upon the Atmosphere. By Charles Daubeny, M.D., F.R.S., Professor of Chemistry in the University of Oxford.*

It was a wise course pursued by the College of Physicians in the case of Dr. Daubeny, to grant him a plurality of offices—professor of botany, when he was already professor of chemistry. It was solicited, we believe, in order to be able to carry on the researches, of which we have here the first fruits; and granted for the sole purpose of effecting a series of experiments, which under other circumstances could never have been performed.

Perhaps, upon the whole, there is nothing very surprising in these results to the lovers of mere novelty, and the "hunters of wonderment." Oftentimes we are led to infer, from incomplete evidence, certain conclusions which are really true; and we are therefore too apt to undervalue the labours of him who demonstrates that truth to us conclusively. It would, however, be easy to show that opinions apparently as well founded as these were, have yet been found utterly false; and for anything we knew to the contrary, before the experiments detailed in this paper, these might have been fallacies as egregious as many others which are now exploded. We trust, however, that Dr. Daubeny will follow up these inquiries with his own characteristic ardour; and complete the experimental discussion of the relation between the chemical and physiological action of plants in combination with those agencies which have any influence upon them.

We can here only give his own synoptical table of the "scheme of experiments," and refer our readers for the details to the elaborate dissertation itself.

SCHEME OF EXPERIMENTS.

PART I.—ON THE ACTION OF LIGHT UPON PLANTS.

- I. Solar light.
  - A. direct,
    - 1. In causing the leaves to emit oxygen, and to decompose carbonic acid …………
      - Immersed in water. Tried with 1. *Brassica oleracea;* 2. *Salicornia herbacea;* 3. *Fucus digitatus;* 4. *Tussilago hybrida;* 5. *Cochlearia Armoracia;* 6. *Mentha viridis;* 7. *Rheum rhaponticum;* 8. *Allium ursinum;* and several species of *Gramineæ.*
      - In atmospheric air. Tried with Geraniums.
    - 2. To become green when etiolated … Tried with Beans.
    - 3. To maintain their irritability ………… Tried with the Sensitive Plant (*Mimosa pudica*).
    - 4. To exhale water by their leaves ……… Tried with Vines, Dahlias, Helianthuses, *Lavatera arborea,* &c.
    - 5. To absorb the same by their roots …… Tried with plants of *Helianthus annuus, Sagittaria sagittifolia,* Vines, &c.
  - B. diffused or reflected. Its influence compared with that of direct solar light in the above particulars. Tried with the Geranium, Myrtle, and Polypodium, so far as regards its relative influence in causing the emission of oxygen.
- II. Artificial light, obtained A. from lamps. Tried by Professor DECANDOLLE.
  - B. from incandescent lime. Tried by myself, but no influence detected.

PART II.—ON THE ACTION OF PLANTS UPON THE ATMOSPHERE.

- I. Proportion between the effects attributable to their action during the night and during the day.
  - 1. During fine weather, and in bright sunshine.
    - Of plants without flowers and with leaves alone, viz.
    - Of plants with flowers and leaves.

		Maximum increase per cent. of oxygen.
Of plants without flowers and with leaves alone, viz.	Cupressus …………	2
	Cedrus ……………	3·75
	Syringa vulgaris …	8·75
	Ditto ………………	6·50
	Pelargonium ………	2·00
	Ditto ………………	5·00
	Crassula, 2 sp. ……	0·00
	Mesembryanthemum	2·40
	Dahlia ……………	3·00
Of plants with flowers and leaves.	Dahlia ……………	3·75
	Helianthus…………	1·00

  - 2. During bad weather, or in diffused light.
    - *Syringa persica* 3 per cent.
    - Geranium, Myrtle, Fern, as noticed above.
- II. Proportion between the carbonic acid absorbed and oxygen evolved. My experiments show that when plants are confined the former is always greatest at first; but this may not continue to be the case after a certain interval.
- III. Greatest amount of oxygen that can be added to the air of a jar by the influence of a plant. My experiments show that at least 18 per cent. of oxygen may be so added.
- IV. At what stage in the scale of vegetable life the function of purifying air stops. Probably where there cease to be leaves.—I have shown that it exists in dicotyledonous and monocotyledonous, in evergreens and deciduous, in terrestrial and aquatic plants, in the green parts of succulents, as well as in ordinary leaves, in Algæ and in Ferns as well as in phanerogamous families. Prof. MARCET has shown that it does not take place in Fungi.

XIV. *Researches on the Integral Calculus, Part I. By H. F. Talbot, Esq., F.R.S.*

On the ulterior object of these researches, we cannot pretend to offer a conjecture; but so far as they at present extend, they are full of interest. We are not, however, without hope that a course of new inquiries is here opened, which will in the end lead to the solutions of various physico-mathematical problems which have resisted the united efforts of all geometers who have hitherto attempted them.

It is familiarly known that every problem which natural philosophy presents to the mathematician is ultimately, and without much difficulty, reduced either to the solution of an equation, or to a series of integrations. So far as "algebraic equations" are concerned, this difficulty has been entirely removed by living geometers, Horner and Sturm. In the case of transcendental equations, no general method of direct solution is known; but the rule of Double Position is found in most cases to be effective,—though the immense labour which it requires is often too great to admit of its application to the cases which present themselves. Much, therefore, remains to be done; and most probably the ultimate method of solution of such problems that will be arrived at is, the invention of methods of developement in rapidly converging series, a few of whose terms may be taken as a sufficient approximation to the sum of the whole.

The differentiation of a function can, in general, be easily effected—as is the case with all *direct* mathematical processes; but the differential expression itself that is given, whether taken at hazard, or resulting from the expression, in mathematical symbols, of a physical hypothesis, may not be such as could have resulted from any direct differentiation. In such case, we may infer at once that its integral cannot be exhibited. Such an expression can, however, always be integrated if conjoined with some other expression of a more or less complicated character. But this does not alter the state of the difficulty, except the expression thus added to the given one does itself admit either of immediate integration or of a similar reduction into an integrable part and another which is similarly reducible, so that by continuing this process we at last arrive at an expression which is wholly integrable.

Of course it is difficult to say, beforehand, what expressions are integrable, since an affirmation of that nature would imply a knowledge of the differentials of every possible expression to be present to the mind,—a supposition that would be perfectly absurd. There is, indeed, a test, whether an expression of several variables be an exact integral or not; but this does not apply to the case of a single variable; and even in the cases to which it does apply, its application is sometimes very difficult, from the different forms under which its results are exhibited. What is still worse, when the integrating factor is found, we have still scarcely any clue furnished by it to the ultimate solution of the differential equation.

As to the method of integration by series, that is, by developing *the function attached* to dx in series, though by different artifices rendered available to a great number of actual problems, it is not in a general condition to be adapted to the wants of the inquirer, inasmuch as the instances are very few in which the series is sufficiently convergent to be calculated in numbers; and in no case does it furnish any material assistance in judging of the general character of the phenomenon it is intended to express, or of the analytical nature of the function itself.

A great number of physical problems, viewed merely as such, are sufficiently solved if we can ascertain the value of the integral, when taken between certain limiting values of the variable. This has given rise to the calculus of *definite integrals*. This method, however, as a mathematical process, is still in its infancy; though some specimens, given by Mr. Murphy in the *Cambridge Transactions*, of attempts at a general method, lead us to hope that it may yet be rendered more perfect and effective than it at first seemed capable of becoming.

The inquiry respecting the algebraic expression of the values of integrals between given limits—or rather requiring the second limit to be found, the first being given—originated with Fagnani, about 1714. He published, in 1715, in the *Giornale de' Letterati d'Italia*, a solution of the problem—given the equation of the parabolic curve $x^n = y$ (where n is 3, $\frac{5}{4}$, $\frac{3}{5}$, $\frac{2}{5}$, $\frac{1}{3}$, or $\frac{1}{4}$) and an arc of it, to find another arc of it, so that their difference may be rectifiable." Again, in 1718, he published a variety of important theorems respecting the ellipse, hyperbola, and lemniscate, in which he showed how to find two arcs whose difference should be a straight line. This gave rise to a more extended inquiry.

The expression $\frac{dx}{\sqrt{a + \beta x + \gamma x^2 + \delta x^3 + \epsilon x^4}}$ does not admit of an exact integral directly obtainable by any known means; but Euler showed that by taking another function in y similar to this in its form, and identical in its constants, the sum or difference of the two might be integrated in an algebraic form. Many important consequences followed from this result, and many attempts to extend the method to other forms of the radical, and to a greater number of terms, have been made by subsequent mathematicians. In 1792, Legendre read to the French Académie a memoir* whose object was to classify and arrange the elliptic integrals which were implicitly contained in Euler's solution. He subsequently expanded this paper, and published these extended researches in a work entitled *Exercices du Calcul Intégral*, in three vols., quarto, Paris, 1811. His tables are peculiarly valuable.

Putting $R = x^n + a x^{n-1} + \dots$ (n being any whole number), and P a function of the same form, $x^m + a, x^{m-1} + \dots$ the lamented Abel of Christiana, published in 1828, gave an expression for the sum of a series of integrals of the form $\int \frac{P\,dx}{\sqrt{R}}$. The steps by which he arrived at his formula were never published.

In 1834, Poisson, in a paper published in Crell's *Berlin Journal*, has considered, several forms of such integrals, which are not comprehended in Abel's theorem; and it hence appears that this celebrated result is not given in all the generality of which it is susceptible.

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* In 1809, a translation of this valuable memoir was printed in Leybourn's *Mathematical Repository; and it is moreover remarkable as containing the first introduction of the notation and terminology* of the differential calculus into English books. This was at least ten years prior to its introduction into the Cambridge works.

This is the state in which Mr. Talbot found the problem;—that is, when he came to examine what had been done by others, though he had obtained his chief results, and was in possession of his general method several years before Abel's theorem was made public. The problem which he here proposes to solve is:—"To find the sum of a series of such integrals as $\int \phi(\mathrm{R})\,d\mathrm{x}$, R being any entire polynominal, and ϕ *any function whatever*."

The history illustrated by examples which he gives of his own progress in these researches, is highly judicious and instructive. It is, however, impossible to give a condensed and intelligible account of his processes within the limits of this Magazine: nor, till the whole series is before us, would it be possible to do the method that full justice which it very obviously demands. Two methods of proceeding have already been developed,—the one of which is founded on a "change of the conditions," shown to be *necessary* in the other for the solution of the problem in all its generality. The whole process is founded on the method of "integrating by parts," a series of symmetrical functions of the assumed variables. Yet we would not have our readers think, because its first principles are known ones, that its results have ever been anticipated, or its current processes ever employed before.

This paper terminates the part; and we would earnestly recommend its attentive perusal to every mathematician who feels interested in the progress of his science. We may state, moreover, that it is not a difficult paper to read. There is none of the quackery of new symbology, or of the mystified and unsatisfactory reasonings which so much disgrace too many of "the most learned" mathematical papers of the present day, to be found in this; and no acquaintance with other writers, beyond the mere elementary ones, is required to enable the reader to comprehend it fully.

ON THE SIGNS OF MULTIPLICATION, DIVISION, Etc.

In our fifth number, (vol. i., p. 291,) we offered a conjecture respecting the origin of the signs + and −. Since then, the writer has received from a friend, the most eminent mathematical antiquary of the present age, and a distinguished professor in the University of Oxford, another conjectural mode of deriving these symbols. An extract from his letter is placed below; and we proceed to lay before our readers the best information that is possessed respecting a few others.

The *Symbol of Multiplication*, ×, was first used by Oughtred; and he prefaces its introduction simply with the remark:—"Multiplicatio *speciosa* connectit utramque magnitudinem propositam cum notâ *in* vel × ; *vel plerumque* absque nota, si magnitudinis unica litera. Et si *signa sint similia*, producta magnitudo erit affirmata: sin diversa negata.

Effertur autem per in.[*]" It probably was used as a variation of the form of the symbol +, the operation of multiplying being merely a substitution for that of addition, in the case where all the numbers to be added are equal to one another. We had, indeed, before we were aware of this passage, imagined it to be a contracted representation of the "hand-in-hand," and to designate perfect union or amalgamation. That passage, however, does not seem compatible with such an hypothesis; as in such case, some remark on the subject would in all probability have been made to point out the views by which he was led to it, and to enforce its adoption by others.

The *Symbol of Division*, ÷, is merely a contracted mode of designating the *positions* of the divisor and dividend in the old Italian mode of operating. It was employed in the first place to concentrate the matter on a *printed* page; as it requires two lines to express it fractionally, and only one to express it by means of this interposed symbol[†].

The *Sign of Equality, in its present form*, =, was first employed by Recorde. He gives his reason thus:—"and to avoid the tediouse repetition of these woordes: is equalle to: I will sette as I often doe in woorke use, a paire of paralleles or gemowe lines of one lengthe, thus, =, because noe two thynges can be moare equalle."—*Whetlestone of Witte*, p. 105. Harriot, also, apparently used it, without any knowledge of Recorde having done so before him.—*Ars Praxis Analytica*, p. 10.

Before this time, and for a long period subsequently on the continent, the symbol of equality, was ∝ , or ꝏ, which is very evidently the initial diphthong æ, of *æqualis*[‡].

The *Symbols of Greater and Less*, viz. > and <, were invented by Harriot, and first appeared in Warner's publication of the *Ars Praxis Analyticæ*, some years after the death of that extraordinary man[§]. See that work, p. 10. They are very appropriate, the point being in both cases directed towards the less quantity, and the opening towards the greater. The sign of inequality, without asigning which is the greater, viz., ≠, is of modern date, and we are not quite certain who was the first to use it. It is merely the sign of equality, "crossed out." Girard used ff and § for greater and less, or for > and <.

As a conjecture respecting the symbol ∽ placed between two quantities to signify their *difference* without assigning which was the greater, we have heard a very eminent scientific gentleman express his opinion that it is the letter *s*, employed as the initial of *subtrahere*: but we rather incline to think it a modification of the manuscript *d*. Of this the reader may easily satisfy himself by writing the small *d* with the

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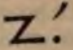
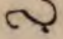
[*] *Clavis Mathematica*, p. 10, *Edit. Quinta*, 1698.

[†] A very ample account of the history of arithmetical operations may be seen in Leslie's *Philosophy of Arithmetic*, or in Peacock's Arithmetic in the *Encyclop. Metropol.*

[‡] A mark very similar to this has been employed *to designate infinity, (see last paragraph,) and precisely* the same form is used even by writers of our own time, as a mark of "general proportion," and is synonymous with, "varies as." This use of it, probably, originated in some fanciful "flourish" carried through the four points of : : or ∺, which are *virtually* used to signify the same relation.

[§] Two great questions in scientific history have heen recently set at rest, in which the name of Harriot stands con-

looped top, and readily trace it through its natural gradations into ∽. It might, possibly, have been from the Greek MS. δ, as it did not make its appearance till the Greek alphabet had become familiar to mathematicians. Of course, the *d* would be used as the initial of the word *differentia.*

Albert Girard employed Recorde's sign of equality, =, for the same purpose; and it is just possible that ∽ might have been derived from this, especially if we suppose that the MS. way of writing the symbol which is printed = was continuous like our letter  It would then easily be rounded into or 

The *Symbol of a Root*, √, is a slight modification of the old manuscript *r*, the initial of radix. The numbers designating *what* root were placed over it, in the earliest MSS., where it occurs precisely as in the present day.

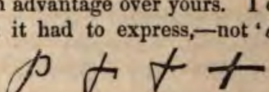
The *Symbol of Infinity*, viz. ∞, is probably a rude sketch of the serpent's folds, the serpent being the familiar emblem of eternity, or endlessness, amongst the ancients. The more appropriate symbol $\frac{1}{0}$, (the more appropriate, because indicating the origin of its occurrence in all algebraic researches,) was first used *instead* of ∞, by the late Baron Fourier, and is now generally employed by mathematicians*.

Sept., 1836.

Note alluded to in the beginning of this Paper.

. . . . "It agrees perfectly with the *general* view which I have long taken of the subject. I differ a little from you, however, in the detail. I think you will find, upon a second examination, that the line above the letters is used, not for the vowel connected with the m and n, but for those very letters themselves. *Diē*, for example, is *diem*, and *tātū* is *tantum.* This strengthens the application of the view; for here we have the symbol itself expressly for the first letter of *minus.*

"The hypothesis which I had framed to myself for the other symbol, was, I own, what appears to have an advantage over yours. I consider it also the first letter of the word which it had to express,—not '*et*,' but '*plus*.' My steps analogous to yours were



Believe me, &c."

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spicuous, by Professor Rigaud, of Oxford. The first, that Galileo had the priority of Harriot in the discovery of Jupiter's satellites; and the second, that Harriot, contrary to the bold and unwarranted assertions of Montucla and other continental historians, did fully understand the nature and management of the imaginary symbol of the roots of equations. See the supplement to the works of Bradley, where lithographic fac-similes of several pages of Harriot's own papers are faithfully given.

* We may remark here, that this property of the cipher, it being the reciprocal of infinity, was well understood by the Indian algebraists, as is well established by Mr. Colebrooke, in his valuable and elaborate work on the Hindoo mathematical science.

A POPULAR COURSE OF GEOLOGY.

II.

Practical Application of Geology.

But what is the use of Geology? This is a question which we have often heard asked, and to which the querists generally reply in the same breath, by denouncing it as a visionary speculation, or, at the best, laborious idleness, productive of no practical results. On this point we are prepared to join issue with these objectors, and to vindicate the utility of our science.

In enumerating the advantages to be derived from it, we shall begin with its economical importance, because the majority of mankind is composed of those who refer all things to this standard. And here we must confess that, as regards utility, as well as the loftiness of their speculations, geologists must be contented to yield the first place to Astronomy. We pretend not to guide the sailor across the deep, and to enable him, by measuring the distance of the moon from some of the fixed stars, to ascertain, within five miles, his situation on the pathless ocean, after he has been months without seeing land; but, upon our own element, the land, we can confer upon mankind benefits of no mean order. We can assist the farmer to fertilize the surface of the earth, so that two blades of grass shall grow where one grew before; and we can impart system to the labours of the miner, so that, no longer groping his way in the dark, or trusting to dreams, to omens, and the divining-rod, he may prosecute, with confidence, and with an approach to certainty, those costly operations which are necessary in order to extract from the earth the treasures which have there been stored up for our use. These treasures exist in sufficient abundance to afford a rich reward to our toils, and, at the same time, they have wisely and beneficently been rendered sufficiently difficult of access, to stimulate industry, and call forth all our energies.

The mineral wealth of the earth has not been distributed through it at random; but each formation, as geologists call a group of strata, is, over extensive areas at least, the peculiar receptacle of certain minerals. Thus, tin is found only in granitic districts, and copper is most abundant in those and the adjoining schistose rocks. That thick formation of limestone, to which the name of Carboniferous has been given, because the great body of the coal-measures rest upon it, is, in England, the chief depository of lead. These metals, with silver, and some others, occur in veins, traversing the strata. Gold, on the contrary, is rarely met with in veins, but is disseminated in small quantities through those rocks in which it occurs, and the principal supplies of it are derived from alluvial gravel, which has resulted from the destruction of those rocks. Platinum and diamonds are likewise found in alluvial gravel. Iron, to which the name of precious might with more propriety be applied than to gold or silver, occurs in the greatest abundance interstratified with coal; so that, by an admirable arrangement of Providence, the bulky ore of this useful metal is found *in juxtaposition with the* fuel and the limestone necessary for *its reduction to the metallic state.*

In the present state of our knowledge, it is too much to affirm that these general rules prevail over the whole earth; but they hold good over extensive portions of the earth's surface, though, even within those areas, there are exceptions to the rule; and the study of the rule and the exception is alike profitable.

Let us take, for example, the case of coal. Though raised from great depths below the surface, coal is vegetable matter, accumulated during the earlier ages of the world; so that we are warming our houses, and lighting our streets, and spinning our cotton, and propelling our steam-vessels, and we may be shot along a railway at the rate of sixty miles an hour, by means of fuel derived from the wreck of forests that flourished myriads, perhaps, of years before the existence of the human race.

Deposits of vegetable matter occur in formations of all ages, but they occur only in thin seams, and in small quantities. The great coal-deposit lies between two formations known by the names of the old and the new red sand-stones, closely resembling each other in mineral composition, though very different in their zoological characters. A few years ago we should have said that this carboniferous limestone was the base of the coal-measures; but Professor Sedgwick has shown that in the range of this formation towards the North of England it becomes a complex deposit, containing beds of shale and sand-stone, with seams of coal, which on the borders of Scotland are so largely developed as to be worked for the supply of the metropolis. It may, therefore, be said, that the great mass of bituminous coal, capable of being profitably worked, lies above the old and below the new red sand-stone. The coal-fields of Brora, in Scotland, and of Whitby, in Yorkshire, can scarcely be called exceptions, though they are situated in a newer group of rocks, called the oölitic. For they afford an inferior kind of non-bituminous coal, worked only to supply a local demand.

In communicating these facts we are obliged to anticipate the knowledge of our readers as to the order in which the strata succeed each other,—

> Things by their names we call, though yet unnamed,

and therefore, perhaps, they can scarcely perceive the full import of these remarks. In that case, we must request them to return to the subject when they shall have become familiar with the names of the formations, and with their order of superposition.

Now, the practical results to be derived from a knowledge of this general rule and its exceptions are these: That searches for coal ought only to be undertaken with the greatest caution, and under peculiar circumstances, in strata beyond the limits of the regular coal-strata, but that in certain situations, where dislocations of the strata have brought to the surface portions of these anomalous coal-measures, and when the carbonaceous matter is seen *in force*, the working of them may sometimes be attended with success in districts which are ill supplied with coal from the coal-measures properly so called. The same remark applies to the lignite, or wood-coal, of the tertiary strata, which is wood partially carbonized, affording a very inferior fuel, which would never be used where the produce of the coal-fields of Newcastle, or Staffordshire, or South Wales, could be easily obtained. In England, which is so abundantly supplied with coal of the best description, lignite occurs but sparingly, but on the Continent it is sometimes largely developed, and extensively worked.

Copper is not generally met with in strata more recent than the old red sand-stone; but there are in England some exceptions to this rule:—the celebrated Ecton mine, in Staffordshire, and the Llandidno, or Orme's Head mine, in Caernarvonshire, being situated in the carboniferous lime-stone. On the Continent, the ore of this metal is worked in a formation even still more recent, the copper-slate of Thuringia being a portion of the new red sand-stone series. The advantages to be derived by the practical miner from a knowledge of these facts are obvious. On the one hand, the general rule will prevent a waste of capital in fruitless searches for certain minerals in strata where they rarely occur, and on the other hand, the exceptions will prevent that slavish adherence to the general rule which would prohibit all attempts to work them in those strata, when the indications are in other respects favourable; and Geology furnishes us with means for discriminating the different strata, and teaches us their order of succession; so that having ascertained to what part of the series a given rock belongs, we know what other rocks we may expect to find above and below it.

Mineral veins, again, are not equally productive through all parts of their course, but masses, or "bunches," as they are called, of ore, are locally distributed through the vein, being connected by thin strings of ore, or by barren portions of the vein-stone, which are only useful as guiding the miner to the richer deposits. His object, therefore, is to arrive at the productive portions of the vein with the expenditure of the least possible labour, on barren ground; and this object he attains by observing the circumstances under which these bunches occur in those particular veins on which he is employed, till, by repeated observations, he discovers the laws which they follow, and knows those parts of a vein which are likely to be most productive. But these laws are not the same in all mineral districts. If, therefore, we remove from any one of our most celebrated mining-counties one of its most experienced practical miners, and if we place him in another, where different laws prevail, for the distribution of ore in veins, and where, though minerals may be abundant, those laws are not yet known by experience, because no mines have yet been worked; all his practical knowledge acquired from observations in his native district will avail him nothing, perhaps will become absolutely injurious, because it will be likely to prevent his paying attention to the suggestions of those who have studied mineral veins on a more general scale. This study forms one department of Geology, and the mine-agent who possesses most of it is most likely to conduct mining-operations with success. A person of this kind is too often branded by the ignorant with the epithet, "theoretical," whereas, he is in fact the really practical man, because he concentrates and combines in himself the knowledge derived from the observations of all practical miners in every part of the world.

We cannot *quit this subject*, without expressing astonishment that *Great Britain, which owes so* much of its greatness to its mineral riches, and where so much property is embarked in mining-speculations, should have been so long without establishments like the *Ecoles des Mines* of the Continent, in which superintendents of mines and working miners might be instructed, at a trifling expense, in the rudiments of Geology, Mineralogy, Chemistry, and other branches of science immediately connected with their occupation. It is to be hoped that the British Association for the Advancement of Science will turn their attention to this subject, and force it upon the notice of the government; for nothing will tend so much to the advancement of science as the union of practical and scientific knowledge in the same individuals. In the depths of the mine, which philosophers only visit occasionally, phenomena of the utmost importance for the elucidation of those hidden laws of nature which they are investigating are daily passing unnoticed before the eyes of working miners. When they shall have become imbued with such a portion of scientific knowledge as will be useful to them in their vocation, and with that love and respect for science which this knowledge will engender, these phenomena will arrest their attention, and though they may not themselves be capable of generalizing from them, they will not fail to bring them under the notice of those who have the ability to do so.

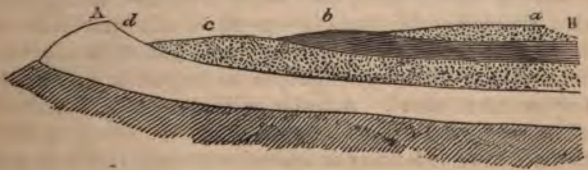
The benefits which Geology can confer on agriculture are neither few nor trifling. Our limits will not permit us to point out in detail how the nature of a soil depends on that of the rocks from the disintegration of which it was derived, nor to show how particular plants affect particular soils, in which, in a state of nature, they exclusively flourish, and in which they flourish most in a state of cultivation, so that by consulting a good geological map, of a given district, we may predict, before we enter it, the species of crops which will be found most extensively cultivated there, and which experience has proved to be best adapted to it.

A due mixture of the earths in a soil is essential to its fertility. The most productive districts of England have been made so by nature, and owe their fertility to this mixture; and it is by copying nature that we must proceed in our endeavours to improve those that are barren. Neither pure clay, sand, nor chalk, afford productive soils. Those are the best which contain a mixture of the three earths, silica, alumina, and lime, with a portion of decomposed animal and vegetable matter. These are the soils, so much coveted by the farmer, which will bear repeated cropping without manure. The principal ends proposed in agricultural improvement are, to render wet soils dry, and dry soils sufficiently moist, to render adhesive soils loose, and loose soils sufficiently adhesive; and the proportion in which the earths above mentioned should be mixed for these ends must depend upon the climate and the substratum. Aluminous, or clayey, soils retain too much moisture, and siliceous, or sandy, soils part with it too rapidly; and a soil, good in itself, may be rendered unproductive by resting on too retentive or too porous a substratum. Draining or irrigation are in such cases the remedy. When one of the earths prevails in a soil to the exclusion of the others, great improvement may be effected by the addition of that which is deficient, and it is astonishing, as M. De la Beche has remarked, that the superior fertility observable along the line of junction of two rocks, occasioned by the mixture of their component parts, has not oftener induced agriculturists to have recourse to various artificial mixtures of the materials of rocks which are adjacent to each other, either as regards the surface, or depth below the surface. There are districts in which such mixtures have been practised with the greatest success; but, in general, farmers rely too exclusively on farm-yard manure. Clay, sand, and limestone, are, nevertheless, mineral manures of the greatest value, and have changed the face of whole districts that were before comparatively barren. By such mixtures the constitution of the soil has been improved,—causing the animal and vegetable manure afterwards applied to be more efficacious.

The clayey lands of Essex have been greatly improved by the use of chalk. This acts upon the land in several ways. It decomposes any free acids, and some acids in combination, naturally existing in the soil, and which are prejudicial to vegetation, and it acts mechanically, by rendering the soil more pervious to moisture, and affording greater facilities for the roots of plants to expand. When chalk is not at hand, a dressing of sand or fine gravel will produce some of these results, particularly if it contains calcareous matter, in the shape of fragments of limestone or broken shells. Clay, again, is equally efficacious on sandy soils, by increasing the power of the soil to retain moisture, and by enabling the roots to maintain themselves firmly in the ground.

In Norfolk and Suffolk, vast tracts of land, which were before incapable of bearing corn, have, by the application of clay, been made to produce good crops of wheat, barley, clover, and turnips. Thus a greater number of cattle are kept upon a given area, and the quantity of animal and vegetable matter returned to the soil is proportionably increased. The mineral manures are, in general, too much neglected, with the exception of lime (the injudicious use of which too often runs into the abuse), and even in those districts where they are applied, they are frequently brought from a distance, when, though not visible on the surface, they exist at a small depth below it, sometimes under the very field for the improvement of which they are required. Now, who, we would ask, is most likely to discover them,—he who never looks deeper into the earth than the bottom of his ditches, or he who studies the position of rocks, with respect to each other, and for this purpose examines every natural section by which they are exposed to view in cliffs and ravines, and every artificial section laid open by mines, wells, and other excavations.

Fig. 1.



Let AB in the annexed diagram (fig. 1.) represent the surface of a district composed, like some of the eastern parts of England, of strata of sand, clay, and gravel, resting upon chalk, all the strata having a slight inclination to the eastward, or towards B: a farmer cultivating the sandy soil at *a*, knows, for he observes it in riding to market, that at a certain distance from his own farm he shall cross a tract of clay, *b*, and that, after leaving that, he shall meet with sand and gravel again at *c*, and that at *d* he shall quit the plain and reach hills of chalk. But he does not know, and he would probably laugh at the person who communicated the information, that all the strata *b*, *c*, and *d*, are to be found under his own land at *a*, at a depth proportioned to the thickness and inclination of the strata, so that a bed which, in one part of an estate will be at a considerable depth, may rise in another very near the surface. But these are points to the determination of which the geologist applies himself, and having ascertained, from the nature of the embedded fossils, that the stratum, *a*, is the crag, and *b* the London clay,—he knows that by proceeding in a direction contrary to that in which the strata dip, he shall meet with the sands and gravels of the plastic clay, as well as with the chalk rising successively to the surface.

A knowledge of the stratification of a country will also prove valuable to those engaged in agricultural improvements, by enabling them to drain their land more effectually, and at less expense, than by the ordinary methods. A good soil, as was before observed, may be rendered unproductive by resting on a bed of clay, which holds up the water, and there may be a porous bed again beneath the clay. Now, if the thickness of the clay is not too great, such land may be freed from excessive moisture by perforating the bed of clay, and leading the surface-water into the perforations.

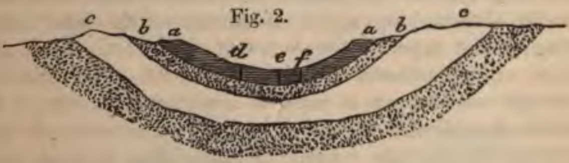
The dip or inclination, and also the "faults" or dislocations of the strata of the district, ought to be studied with the same view. These faults are fissures which traverse the strata, and produce two opposite effects with respect to drainage, according to the nature of the substances with which they are filled. Sometimes they are pervious to water, and then they act as natural main drains, into which subordinate drains may be turned. In other cases they are filled with clay, and then they act as mutual dams, holding up the water in the strata to a higher level on one side of the fault than on the other. A fault so filled is often traceable on the surface by the land-springs which break out along its course, arising from the pent-up water struggling to escape wherever there is the least resistance; and cases will occur in which, by piercing through the fault, vent may be given to the water which before burst out at several smaller openings, and the land may thus be laid dry at a cheaper rate than by contending with each spring individually. This water, which was injurious in one place, will sometimes be in sufficient quantity, when thus collected into one channel, to improve another part of the estate, by being employed in irrigation, or by being made to drive an overshot wheel. Artesian wells have been bored to obtain a supply of water for both these purposes.

The nature of a country is sometimes such that it is destitute of rivers and surface-springs, and then the population are dependent for a supply of water upon that which can be obtained by means of very deep wells, or upon that which falls from the atmosphere, and is collected in ponds or tanks. The water derived from the former source is obtained at a great expense; that derived from the latter is by no means wholesome, and is liable to fail in dry summers. In such seasons, we have seen the population of an extensive parish almost fighting for the green unwholesome water of a muddy pond; and cases have occurred, in which it has been necessary to drive the cattle so far to drink, that by the time they returned, they were as thirsty as when they started. Few greater benefits could be conferred on a district so circumstanced, than the establishment in it of Artesian wells.

These derive their name from the district of Artois, in France, where they are supposed, though it appears erroneously, to have been first used. Artesian wells are nothing more than perforations a few inches in diameter, made through the strata with the ordinary boring-tools, and their action arises from the natural tendency of water to find its level.

It is only under certain conditions of geological structure that they can succeed; and the structure of the district in which it is proposed to introduce them ought to be thoroughly investigated before any borings are attempted, because, though one of these wells may be made for one-third of the cost of an ordinary well of the same depth, yet it will be perfectly useless, though a water-bearing stratum should be penetrated, if the water will not rise to the surface, the orifice being too small to admit of its being raised by mechanical means. If a reservoir of water were formed on the side of a hill, and a leaden or other closed pipe were laid from it to a hill on the opposite side of the valley, the water might be conducted in this pipe to the same height as that of the source, and wherever a hole was made in the pipe, the water would gush out and endeavour to attain the level of that at the two ends. The geological structure answering to these conditions is shown in the annexed diagram (fig. 2.), which represents the stratification of the vale of London. *a a* is a retentive stratum at the surface (in this case the London clay);

Fig. 2.



b b a bed of gravel and sand below it, both of them resting upon the chalk, *c c;* and the whole series having been thrown, by some subterranean movement, into a curve, concave on the upper side. This, in geological language, is the basin-shaped structure. The porous bed, *b b*, fulfils the office of the closed pipe before mentioned. The water which falls on the chalk-hills, *c c*, is absorbed by this porous bed, and prevented from escaping by the bed of clay above, and by the chalk below it; and if the clay be pierced by small borings as at *d*, *e*, and *f*, will rise to the surface, or to heights above the surface, proportioned to the elevation of the source, and to the obstacles which the water meets with in percolating through the bed, *b b*. There are other modifications of this structure which need not be noticed in a brief sketch like this.

There are few persons conversant with rural affairs, who are i aware how much good roads improve the value of property by facilitati the conveyance of the produce of the land to a market, and either the improvement of old, or the formation of new roads, a knowledge the strata over which they are formed, is of the utmost importance. T durability of a road depends in a great measure upon the materials e ployed, and upon the solidity of its foundation. These points are, general, too much neglected by those to whom the formation of road intrusted, the chief objects of attention being the shortening of distan and the reduction of acclivity; and to obtain these a road is often carr over strata affording an unstable base, and liable to frequent land-sl Soft and bad stone is also frequently employed, because it is nea hand, when better materials might be brought from a distance by me of canals and railways, at a small extra expense, which would be in end more economical, because of their greater durability, and beca when used, they afford a good instead of a bad road. On the o hand, stone of inferior quality is often brought from a distance of sev miles, when better exists much nearer; not quarried, it is true, visible on the surface, but easily discoverable by the eye of the geolo Hardness is not the only essential in a stone employed for the repai roads. It is required not only to resist friction, but also the crusl force of heavy carriages, moving with considerable velocity. H a certain degree of toughness is necessary to durability. Yet when l and tough materials are both at hand, the latter is often neglected the former.

There are situations in which flints from the chalk, and chert f the green sand, might be obtained for the roads at the same cost. chert, as the toughest, is the best; yet surveyors of roads, looking onl hardness, or perhaps merely adhering to established custom, alr invariably prefer the flints. Masses of greenstone, again, are of freq occurrence in the midst of granitic districts. Granite is reduced powder under the crushing action of wheels much sooner than gr stone, owing to the superior toughness of the latter, which arises f the presence of the mineral called hornblende. This mineral en largely into the composition of most of the trappean rocks, a nume family, which, though differing much in external aspects, all possess a greater or less degree, the toughness essential to a good materia roads. We will illustrate this subject by one or two examples.

The improvement which has taken place of late in the road the neighbourhood of London, must be well known to most of readers. Much of this has been effected by reducing the surface better form, and by applying the materials used in a more judi manner; but much has arisen from the selection of better materials

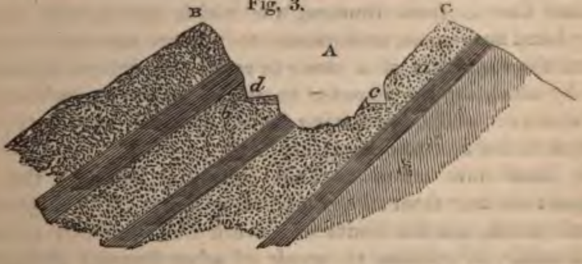
Under the old system, these consisted exclusively of gravel r in the neighbourhood, much mixed with sand and clay, from whi was difficult effectually to clear it. In mineral composition, it con chiefly of flint, but the pebbles being for the most part round, could form so solid a mass as a layer of angular pieces of stone. The firs provement was to substitute for this gravel broken flints from brou [illegible] river Thames. Possessing hardness only, without t

ss, their brittleness was found to be an objection against them, and
anite was substituted. This was found more durable than flint, but
ss so than a hornblende-rock from Mount Soar Hill, in Leicestershire,
hich is now chiefly employed, and with manifest advantage.

The surface of the counties of Lancashire and Cheshire is occupied
iefly by the new red sand-stone formation, which affords no road
aterials, but stone of the most friable kind. The roads of this extensive
strict are now supplied with a trap-rock from Penmaen Mawr on the
ast of Caernarvonshire, and the quantity used is so great that even at
e low price at which it is supplied, the persons who have taken a lease
these rocks are realizing large profits. Stone of the same kind occurs
many parts of the Snowdonian chain, but nowhere so near the sea,
cept in Caernarvon Bay; and even the small additional freight which
ust be paid for its conveyance thence, is sufficient to prevent it from
mpeting successfully with the stone of Penmaen Mawr. Whoever,
erefore, shall succeed in discovering within the district thus supplied a
ass of trappean-rock, so situated as to be easily quarried, may be
sured that he has made a valuable discovery. Mr. Murchison has
own that in a part of Shropshire trappean-rocks have burst through
e red sand-stone, and it is by no means improbable that the action
ay have been prolonged into Cheshire. It sometimes happens that
ly a small portion of trappean-rock rises to the clay, when there is a
rge mass at a small depth below the surface; and the surface-appear-
ces are also such as to deceive a person in search of road-materials
ho was unpractised in geological investigations; for some of the hardest
d toughest of these rocks decompose by long exposure to the atmo-
here, presenting the appearance of an incoherent bed of earth, on
moving which, the unweathered rock will be found to afford a stone
ell adapted to the repair of roads.

Attention to the direction in which the strata dip will frequently
ve considerable expense, both in the first construction and future main-
nance of a road, when it is carried along the side of a hill through a
ratified country.

Fig. 3.



Let A (fig. 3.) represent a valley between the hills B and C, com-
osed of many alternations of shale and sand-stone, having a steep dip;
that had the beds of B not been denuded, they would have covered
ose of the hill C. Let *a* and *b*, moreover, be two beds of loose sand-
one, on opposite sides of the valley, with beds of clay or shale beneath
em. If a road be cut at c, the water percolating through the sand-

stone, and prevented from descending by the retentive bed below, will have a constant tendency to throw down upon the road portions of the rock above; but no such effect will be produced if the road be cut at nearly the same height on the opposite side of the valley, at *d*, where the strata dip *into* the hill instead of *out of* it.

Every architect and civil-engineer ought to be a geologist, for the study of the structure of rocks, and of the situations in which they occur, would frequently enable them to select more durable stones for building than those usually employed. These remarks apply to public works rather than to private edifices, for under the present system the houses in most of our great towns are built by speculators who have no permanent interest in them, and they are built for profit, on a calculation, nicely adjusted to the length of a lease, which they are not intended to outlast. But when a proprietor builds upon his own estate, he ought not, from motives of injudicious economy, to use stone liable to rapid decomposition, merely because it can be worked at less cost than another which is more durable, and, above all, when a nation builds, which builds for posterity, all such paltry parsimony ought to be discarded. Yet some of the finest of the public buildings in London, less than two centuries old, are hastening rapidly to decay, from having been constructed of a calcareous sandstone, soft when first raised, and thence easily worked, hardening as it loses its original moisture, but liable again to imbibe water, and therefore easily affected by frosts. An improvement in this respect has lately taken place in the substitution of granite for sandstone in many of our public works; but the indiscriminate use of this material may be attended with bad effects; for some varieties of granite, like the trappean rocks before alluded to, though hard and difficult to work, when first raised, afford a bad building-material, from the rapidity with which they decompose. This depends on the state of the felspar they contain. Rocks, in which the felspar exists in a compact state, are in general more durable than those in which it is found in coarse crystals. Some sandstones, again, which have a siliceous cement, are very durable. The weathered surface of a rock will often give a better insight into its mineral structure than can be obtained from a fresh fracture, for which reason, the geologist, in selecting hand specimens, endeavours to procure one such surface; and in general the capability of a stone to resist atmospheric agency may be learned by studying the manner in which it resists such action in its original situation, and by observing which of the ingredients of a compound rock is the first to decompose.

We have now taken a rapid glance at some of the practical advantages resulting from the study of geology. The subject is by no means exhausted, but the limits of a work like this will not admit of further details. It remains to speak of other benefits arising from the cultivation of the science, less direct, it is true, and, to some minds, less obvious, but not the less important, because they cannot be made to enter into a calculation of pecuniary profit. As an employment for the mind, and an exercise of the reasoning powers, geology may be placed in *the first rank*, second only to the exact sciences, and in some respects *superior to them*, because it requires the exercise of those faculties, and employs that kind of reasoning, for which we have daily need in the conduct of human affairs. The practical application of the mathematics is limited to a few professions. That is not the object with which they are studied by one in an hundred of those who devote so much time to them. They are studied in order to strengthen the mind by accustoming it to habits of attention and investigation, and even when cultivated with this view, doubts have been entertained by some, whether they may not be pursued too exclusively. It has been asserted, that the mind may become so habituated to that strict demonstration which the abstract sciences require and afford, as to reject other truths incapable of the same kind of proof. And yet on most of the questions on which we are called upon to form a judgment as to matters-of-fact, past, present, and to come, mathematical reasoning is inapplicable, and we must be satisfied to arrive at a moral certainty to be attained by weighing evidence and balancing probabilities. This is the highest degree of proof of which most geological inquiries are susceptible, and this is the mode in which they are conducted. We observe the changes taking place around us in the organic and inorganic world, and we trace effects up to their causes; and then comparing like things with like, we infer that similar effects were produced by similar causes in the ages that are past. In this investigation we have often to weigh the conflicting evidence of apparently irreconcileable phenomena, we are perpetually seeking analogies, or detecting differences, or combining and generalizing scattered facts with which observation has furnished us. Occasionally, too, we find ourselves obliged to retrace our steps, compelled by new facts to abandon generalizations, founded on imperfect induction. Surely all this must tend to produce habits of acute observation, patient investigation, and salutary caution, in suspending our judgment in the absence of complete and satisfactory evidence; habits, these, of the greatest value in the affairs of life, and we must be most unapt scholars if we do not also learn from it this great moral lesson—that it is possible to acknowledge ourselves to have been in error, without either compromise of dignity or loss of strength.

Geology possesses this advantage over most other sciences, that with discipline for the mind, it combines exercise for the body. It has power to preserve or to restore the "mens sana in corpore sano." One at least of our most celebrated geologists applied himself to this pursuit, as a remedy for disorders, brought on by too close an application to sedentary studies. Health appears to be a privilege almost peculiar to the geologist, among all the sons of science. The mathematician undermines his constitution over his diagrams and his equations—the chemist inhales poison, amidst the fumes of the laboratory—the anatomist risks his life, amidst the horrors of the dissecting-room,—but the geologist enjoys the robust health of the sportsman, or the peasant. He breathes the purest air of Heaven, amidst the loveliest and the sublimest scenery of nature, whilst exploring those mountain-recesses, where her mysteries are best revealed; hardy, active, and enterprising, he ranges through all the realms of Europe—now on the summit of Ben Nevis—now in the caves of Staffa—now among the wilds of Connemara, and now in the valley of the Arno. He has examined the extinct craters of the Rhine, and the crater of Etna, still in full activity; his hammer has been heard among the pine-forests of Norway, and he has looked down from the Alps, and cried with the poet—

> "Creation's heir—the world, the world is mine!"

He sees mankind under various forms of government and religion. He holds intercourse with men, whose sole bond of union with him is their ardour in the cause of science. On all other points their opinions differ, and even on some of the controverted matters of geology they are ranged on opposite sides. Hence, he is trained to habits of moderation and forbearance, and learns to emancipate himself from the trammels of party. Those ancient convulsions to which the globe has been subject, and those physical features which they have impressed upon its surface, are not confined within the narrow conventional limits of tribes or nations; to study them, he must have a free and unmolested passage through all the kingdoms of the earth. Hence, those bad passions which array nation against nation, injurious as they are to all, have this additional evil for him, that they retard the progress of his science, which requires for its developement nothing less than the peace of the world. The subjects of his investigations are so various, and the questions to be solved by him are so complicated, and so connected with all the kingdoms of nature, that he is prevented from devoting himself too exclusively to any one branch of study; he finds at every step that he needs the aid of fellow-labourers in other departments of the great field of knowledge. Hence arise new sympathies; and there are many who can enumerate among the advantages of geology, the warm and enduring friendships to which it has given birth. In the roving life of the geologist, there are charms unconnected with those of science: he mingles in his rambles with men of all ranks, and of various characters, and thus gains an opportunity of studying human nature, whilst he obtains fresh knowledge of his profession. The personal adventures of a geologist would form an amusing narrative. He is trudging along, dusty and weather-beaten, with his wallet at his back, and his hammer on his shoulder, and he is taken for a stone-mason travelling in search of work. In mining-countries, he is supposed to be in quest of mines, and receives many tempting offers of shares in the "Wheal Dreany," or the "Golden Venture;"—he has been watched as a smuggler; it is well if he has not been committed as a vagrant, or apprehended as a spy, for he has been refused admittance to an inn, or has been ushered into the room appropriated for ostlers and postilions. When his fame has spread among the more enlightened part of the community of a district which he has been exploring, and inquiries are made of the peasantry as to the habits and pursuits of the great philosopher who has been among them, and with whom they have become familiar, it is found that the importance attached by him to shells and stones, and such like trumpery, is looked upon as a species of derangement, but they speak with delight of his affability, sprightliness, and good-humour. They respect the strength of his arm, and the weight of his hammer, as they point to marks which he inflicted on the rocks, and they recount with wonder his pedestrian performances, and the voracious appetite with which, at the close of a long day's work, he would devour the coarsest food that was set before him.

Geology can only be studied thus by those who are gifted with leisure and affluence, or by those who follow it as a profession; but, on a less extended scale, it is available to the man of business and the man of humble income. The rich and the unemployed will find in it a substitute for those artificial excitements, those frivolous, and often vitiating and ruinous pursuits to which they are driven as a resource against *ennui*, and he who is engaged in active pursuits, may resort to it as a relaxation from toil and ease. If a state of total inactivity is unsuitable for man, so likewise is a state of unremitting labour. Rest and recreation we all require: change of employment is often equal to repose; and where can we find employment or recreation so refreshing and invigorating, both to mind and body, as this? What can be more delightful than to exchange that conflict of passions and interests, which beset the man of business in his intercourse with men, for that calm, yet exciting interest, arising from converse with nature, and the acquirement or discovery of truth?

Geology has fields of research suited to every labourer, and to every capacity. On some of its investigations, the highest intellectual powers, and the greatest acquirements in abstract science, may be brought to bear, while many of its problems may be solved by any one who has eyes and will make use of them. Extensive travel is requisite to afford comprehensive views of the structure of the earth, and to prevent our generalizing from too limited an induction; but he whose travels are confined within his native country, or within a circle of twenty miles round his own house, may add much to our knowledge. Nor is this class of observers by any means the least useful. He who makes a hasty excursion into a district, can give but a general outline of its structure, leaving many important points of detail to be filled up by resident observers. If we visit the same cliff or the same quarry daily, for years, we shall, at every visit, be rewarded with something new; and there are few districts barren in objects of geological interest, however deficient they may be in the beauties of picturesque scenery.

ON THE USE OF TERMS FROM THE GREEK AND LATIN LANGUAGES IN SCIENTIFIC NOMENCLATURE.

THERE exists, at present, a strong desire to dispense, as much as possible, with terms and names in science taken from the Greek and Latin languages: it is alleged that by so doing, the attainment of knowledge will be facilitated to those who have not the advantage of a classical education; and that science, being divested of what bears the appearance of an ostentatious display of learning, will present herself in a more attractive guise, and secure a greater number of admirers. It is also considered as a national reflection that we should, in any case, have recourse to other languages, when our own is so copious and expressive. We are *frequently reminded* in a lame metaphor of that "well of pure *and undefiled English,*" from which our ancestors drew—not water—but words, till the example of a master-mind, towards the close of the last century, caused an influx into our vocabulary of Latin, to the exclusion of genuine Celtic or Teutonic.

This is not the place for any discussion on the last allegation, which would properly pertain to a philological journal, or to one devoted to general literature, rather than to science; nor should we have mentioned it, if we did not conceive that it is presumed, the advantages which would result from the banishment of these exotics, and the restitution of their hardier predecessors would be equally felt in the language of science, as in that of literature. We cannot, however, refrain from observing, that our language is essentially a mixed one, and that even if we were to purify it of all the latinity which has been introduced into it since the period alluded to, there would still remain a great leaven, which had been incorporated in the form of Norman French, and that the "pure well" was but a turbid compound of different Celtic dialects, with a large proportion of that of ancient Italy.

Leaving, however, this consideration, we wish to point out the proper bounds to the proposed reformation, and though we fully concur in the desirableness of divesting science of all extrinsic difficulties, we feel persuaded that this method of so doing eventually tends to produce a worse evil than that we would obviate; and that while we endeavour to shun what may appear as pedantry, we shall, if not timely warned, fall into the more dangerous errors of inelegance and inaccuracy.

Some persons have proposed, as a part of the plan of restoration, to extend the privilege of retaining the Latin or Greek root of a scientific term, only giving it an English termination, and forming nouns as we have always been in the habit of forming adjectives. Thus, since custom has sanctioned the use of such terms as *vertebrated*, or *molluscous animal*, *exogenous* plant, &c., they would adopt *mammals*, *cephalopods*, *mollusks*, *exogens*, *bracts*, &c. But as these terms have neither euphony nor elegance to recommend them, it should be distinctly shown that they possess some decided advantages: intelligibility to the unlearned is certainly not among these; for unless the meaning of the root be understood, the vernacular termination will be of no avail, and such jargon as *edentate* or *rodent mammal*, *acephalous testacean*, *glumaceous endogen*, *&c.*, is in no way preferable to the analogous designations with their proper endings.

It is the deficiencies of our own language that necessitate the adoption of foreign words, to furnish names for newly-discovered natural laws, principles, or bodies, inorganic or organic, or for instruments newly contrived to aid us in our researches. The character of the languages derived from the Celtic or Teutonic, is to possess but comparatively few roots, and to allow of an indefinite number of combinations of these to express compound ideas. But however forcible and expressive association may render these compounds, their being such is obviously a source of ambiguity, and prejudicial to the precision and brevity so desirable in everything connected with science; for, in the arithmetic of language, the shortest word is not always that which consists of the fewest syllables; and, in fact, the learned name is, in most instances, absolutely shorter than any equivalent English compound term.

If *weather-glass* be employed instead of *barometer*, we may gain a syllable, but at what a cost! And if we translate the word by *weight-measurer*, there is no gain whatever. What vernacular terms could be found to express oxygen, chlorine, hydrogen, &c.? and still less the significant compounds hydrochloric, hydrocyanic, protoxide, deutoxide, &c.

There is no ground for apprehension that any undue number of foreign words will be introduced, because there is no inducement but necessity to employ them; all languages were formed, and even matured, in the infancy of *science* of every kind, all, therefore, must be extended as new ideas are acquired, or new objects require naming. Viewed with regard to these wants of science, ours is a barbarous and impracticable language, and does not admit of the formation of brief and efficient compounds; and if we were to insist on the adoption of native, to the exclusion of foreign terms, knowledge would receive a check which would be nearly fatal to its progress. The communication of knowledge depends on the right use of correct definitions or descriptions, which ought to be as brief as is compatible with precision and accuracy; no word employed, should, if possible, be ambiguous, or have more than one signification, but the principal roots of any language, from their early and constant use, have become associated with many trains of ideas, which being inevitably excited when the word occurs, its use is incompatible with the singleness of meaning which is requisite to a good description[*].

The first essential step the student must take, especially in natural history, is the attainment of a correct conception of the principles of classification, without which he can only acquire isolated facts, burdensome to his memory. He must understand the nature of *genera* and *species*, and be impressed with the incalculable advantage, in point of brevity of definition, afforded by the combination of the generic and specific name to designate a natural object; this combined name being in fact a *memoria technica*, to remind him of certain properties and qualities of the individual, as the more comprehensive collections of genera into orders and classes are the means by which he is reminded of their mutual relations. It is of course of little importance what words may be employed for this purpose, but they should be euphonous and easily pronounced, and there should be some consistent plan in their adoption: if the advantages of employing the generic and specific name be admitted, the more precise and distinct these are, the better. To substitute three or more separate words, where two are sufficient, would be anything but desirable; and this would be the case in a majority of instances, if an endeavour were made to dispense with Latin or Greek names.

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[*] It will not be useless to give a few examples, taken at random, of the ambiguity which would arise from the use of trivial names, which were given before science had pointed out the affinities of natural objects. The name *violet* is well adapted for a generic term, but it has been given in two or three instances to plants which have not the slightest affinity with a true violet, as the *Calathian violet* (Gentiana pneumonanthe), the *dogs'-tooth violet* (Erythronium dens canis), &c. The term *Nightshade* is given to two distinct genera, and this is the case in innumerable instances. There are between thirty and forty plants designated by names compounded of the celtic root *wort*, as star-wort, mad-wort, spleen-wort, spider-wort, &c. &c.; and in several cases there are three or more plants to which one of these names is given, with some other distinguishing term, though the plants have no real connexion; thus we have *Star-wort* (Aster), *Water Star-wort* (Callitriche). Now, is it not obvious that, if these names were retained, the mind would naturally associate the plants, *from the similarity in their appellations?*

A living poet has written some beautiful verses "to the Celandine," and by his description, it is clear he did not mean the *celandine* (chelidonium), but the *pile-wort* (ranunculus ficaria). We need hardly say that we are far from proposing to banish from fiction those popular names met with in the greatest works of genius, but merely to point out a striking case of error, arising from the use of trivial names.

Captain Back, in compliance with popular usage, says in his narrative, "several *mice* were seen to-day," and immediately explains that the animals were lemmings. When housewives and farmers only thought of depredations on their bacon, cheese, and corn, they called all animals guilty of these malpractices, *rats*, and *mice*, and *sparrows;* but what should we gain by endeavouring to frustrate the labours of naturalists, who, on careful comparison, have instituted the genera, Arvicola, Spermophilus, Emberiza, &c., by again confounding them under the name of *mice* and *sparrows;* or would the vernacular terms, *field-haunters, seed-lovers*, as literally translating the new generic terms, be advisable, either for brevity or euphony?

When a knowledge of even the elements of science was by no means common, it was possible for a person to acquire the reputation of learning, by a pedantic use of *hard words*, as they are facetiously termed by those who do not, as well as by those who really do, understand them. The ridicule so justly excited by this practice, made all true philosophers anxious to remedy an abuse which might be prejudicial to the diffusion of sound knowledge, for some of the ridicule was intentionally transferred to science herself by those who at all times are interested in preventing their fellow-creatures from becoming wiser or better. But now, when the elements of science form part of general education, and when considerable knowledge is no rare endowment, the pedantry alluded to is seldom had recourse to, and the same cause for proscribing terms borrowed from the learned languages no longer exists, while the advantages derived from their use, as convenient symbols in classification, at least, become daily more apparent.

Floriculture and shell-collecting having, for very sufficient reasons, been always popular amusements, the use of Latin and Greek names in the branches of natural history with which these pursuits are connected, has more especially excited animadversion, from the cause above alluded to; but if the scientific names be invariably employed, their use will cease to excite surprise, this is gradually becoming the practice, from a perception of the unfitness of trivial names to meet the wants of even a slight knowledge; and we are endeavouring to prove that this habitual use of the scientific name should be inculcated by all who are aware of the importance of correct views of the complicated relations existing among organic beings.

Why, at the present day, should a person desirous of studying botany rationally, be exhorted to use the term *evening-primrose tribe* instead of *Onagrariæ?* It is true he would use this last word merely as a symbol, which might recall to his mind certain properties, and of the derivation of which he might be ignorant; but the employment of the other would demand an extra effort of attention to divest it of its false and primary meaning, the order in question having nothing to do with a primrose, and the name evening primrose is identified with only one genus which is already far more commonly known by its correct appellation *Œnothera.* Since it is impossible to discover or apply English names for the number of species and genera discovered in the present century alone, t is surely by no means desirable that the naturalist should employ an English name for some orders or genera, or species, and Greek and Latin ones for the rest. Why not, for the sake of uniformity alone, habitually use the latter on all occasions, since the languages which furnish them are exhaustless in their stores, and have the additional recommendation of being universally understood.

If the advocates for vernacular names, admitting the necessity for consistency or uniformity, and the advantages last mentioned, should yet prefer the attempt to adopt English names in all cases, they would soon find the plan impracticable. Suppose, for instance, we were to adopt the trivial name *Speedwell* for the generic term instead of *Veronica*, though there would be no difficulty with such specific names as *Virginian*, *Siberian*, *tall*, *lofty*, *hoary*, &c., how are we to adopt concise and appropriate English synonyms for *peregrina*, *decussata*, *crinita*, *crenulata*, *scutellata*, &c.; we must either substitute an English termination for the Latin one, a practice, which as we have shown, has nothing to recommend it; or we must adopt some awkward periphrasis of several words, and when we have done this, as the idiom of the language requires the adjective to precede the noun, contrary to the principles of classification, which require the generic to precede the specific name; we must either violate this rule, or adopt such inversions as *Speedwell panicled*, *Speedwell many-stalked*, &c.

In the animal kingdom the same incongruities would arise, enhanced by the more powerful effects of early associations connected with names habitually used before classification was thought of; associations, too, usually repugnant to true science: thus the genus, *Canis*, would be divided as follows, Dog-dog, Dog-fox, Dog-wolf, Dog-hyæna, &c.

One inevitable result of improved knowledge of the relations of organic beings is the continual establishment of new genera, or the division of one into several; if the Greek and Latin languages are employed to furnish appellations for these, we are enabled by their exhaustless fertility to adopt such as shall have a reference to the original group of species requiring re-arrangement, and the spirit of classification is preserved in the change. The science of conchology affords pregnant examples of what we allude to.

The genus, *Venus*, is now broken up into ten or a dozen intimately related to it, and yet absolutely requiring to be separated from it; how elegantly and how philosophically are the new generic names furnished by the goddess's Greek synonyms, *Dione*, *Aphrodite*, *Astarte*, *Cytherea*, *Cyprina*, &c. In what manner could we accomplish this in English?

When genera, intermediate to *Cardium* and *Venus*, were discriminated, how correctly and briefly did the factitious compounds, *Venericardium*, *Isocardium*, &c., express the new-found relations. Compare with these the absurd barbarous designations, *stone-piercer*, *gaper*, *razor-sheath*, *top-shell*, *kneading-trough*, *ear-shell*, *barnacle*, &c. &c., which are to be met with in popular works on conchology, and even in our national museum; and when English names are not used, foreign ones are adopted, which can possess no advantage over Latin or Greek appellations, such as *Cowry* instead of *Cypræa*.

It will be said that custom must determine after all in these matters; and that while it is the custom to call a Campanula by the *hard word*, and a Tropæolum by the *wrong word* (Nasturtium, which is the name of the water-cress), no reasoning will induce a change; we are far from flattering ourselves that any observations of ours will have this effect, nor is it a matter of the slightest consequence by what name an object is designated, when it is spoken of without reference to others; but the attempt to restore ambiguous trivial names in scientific works, while we are endeavouring at the same time to inculcate the necessity for an accurate discrimination in minute particulars of habits, structure, properties, &c., of animals, must have a mischievous tendency equally to knowledge and to taste.

Suppose, for example, that English names were adopted, and that desirous of avoiding such barbarisms as mammals, exogens, &c., we translated the classical names; the animal, Agouti, which is of the order Rodentia, class Mammalia, would be described as belonging to the order *Gnawers*, class *Sucklers*. Admitting this phraseology to be desirable, what must we do to define a *slug?* To be consistent, we must call it a *shell-less, land, gill-less, belly-footed molluscous animal*, winding up with an adjective, for which no human ingenuity can find an English equivalent in less than ten words; would it not be better in every respect, even in popular works, to define the creature as a species of the genus *Limax*, of the section terrestrial *Pulmonia*, class *Gasteropoda*, division *Mollusca?* for it is clear that the person who could really comprehend the English definition, must also be sufficiently master of classification to comprehend the latter; and if he did not know the etymology of gasteropoda, he would at least have become familiar with it as a term for a large class of Mollusca, the character of which is to have the organ of locomotion on the part analogous to the belly of more perfect animals.

Our illustrations have been entirely drawn from natural history and physics, because it is in science alone that we think the adoption of vernacular terms to be particularly deprecated. With regard to mere technical words in the arts, custom and the good taste of individuals are sufficient guides as to the propriety, or otherwise, of adopting Greek and Latin names. We smile at the appropriation of *Caminology*, by the French, to express the trade of a chimney-doctor, or of lithologic arts to designate those of a bricklayer and mason; but it would be difficult to offer any valid reason against their adoption, while so many others, not more necessary, are tolerated; why do we ridicule an "Emporium for Blacking," and not "a Bazaar?" And are we not guilty of downright nonsense in styling a collection of foreign animals placed in a pleasure-ground the *Zoological* gardens? The fact is, it is the worth of the subject that sanctions a name in one case, and makes it contemptible in another; but being willing to leave these comparatively unimportant matters to a censor of taste, we hope that science will be left to recruit its nomenclature from those languages in which it drew its first breath, until our own undergoes such a revolution as will better fit it for the purpose,—a revolution which every one would deplore, but which will be accelerated by injudicious advocates.

ON THE ROUGH DIAGRAMS REPRESENTATIVE OF SPHERICAL FIGURES.

THE principles on which the circles of the sphere should be represented, are very simple; and yet the *practice* of mathematicians, who, of all men, ought most to attend to such matters, seems to indicate a total absence of all principle whatever in sketching their diagrams. The distorted and unmeaning figures which are often given to illustrate spherical problems, are sometimes really painful to look upon; and this not only in the drawings of the young, but of the most able mathematicians. I hope this will be a sufficient apology for the few following remarks.

The best mode of representing spherical figures, except when a particular mode of projection is implied in the mode of reasoning, is the common perspective representation on a plane at right angles to the line drawn from the eye to the centre of the sphere. A case of this is the orthographic projection; and the limit on the other side is the stereographic. The latter is too much distorted to furnish any idea of the magnitude and figure of the curve projected; and perhaps the orthographic is of all kinds the best; though the perspective taken with the eye at the distance of fifteen or twenty times the diameter of the sphere may do very well. In all cases where the projecting point is without the circle, the figures into which the circles of the sphere are projected, are *ellipses, and they are all concave towards the centre of the sphere.* Moreover, the more inclined the planes of the circles are to the principal line of vision the greater will their minor axes become.

Those great circles which pass through the point in which the principal line of vision cuts the surface, will be projected into straight lines —a construction which should, except several such circles, symmetrically related to the figure, pass through one point,—be generally avoided, as it renders us liable to mistakes, when actual straight lines occur in the investigation. This precaution is unnecessary when less circles pass through that point, as there is then no liability to mistake, these being projected into ellipses.

The parts of the figure which lie on the hidden hemisphere should be dotted, and form continuous ellipses with those on the visible hemisphere; and it would be well to mark the corresponding points with the same letters, accenting those on the dotted part of the figure. This is in strict accordance with the practice of the best French writers on Descriptive Geometry.

When the figures represented are other than circles, the best mode of marking a few leading points of them through which the projection is to pass, is to draw radiating great circles from the upper point of the great circle parallel to the plane, and projecting the points in these upon the paper as nearly as can be done by estimation, trace the curve through them. Of course, if we wish to have the figures constructed accurately, we must proceed in the usual manner taught in works on Perspective: but as this, especially during the study of a proposition, is altogether unnecessary, and as it is desirable that we should, at least, approximate in general character and appearance to the thing intended, the preceding remarks may not be altogether without their use.

Vicarage, Overton, Sept. 16, 1836. P.T.

MISCELLANIES.

English Industry in Scientific Research.

"Of the fifty-five simple substances recognised in physics, twenty-two, and of the forty-five metals, seventeen, have been discovered in England." Our countrymen may plume themselves, if they please, on this fact. We sincerely wish that they could lay claim to an equal proportion of the whole of the great physical laws, and of the formulæ by which those laws are expressed,—the results of profound analytic investigations based on carefully-repeated experiments, and requiring more intensity of mental exertion than the discovery of a new body, which is most commonly fortuitous.

New Method of cultivating the Cerealia.

MM. Edwards and Collin, in a joint memoir on agricultural chemistry, have shown, that by sowing our principal species of cerealia in the summer season, a profitable harvest of straw, or fodder, may be obtained the first year, the plants not flowering, owing to the elevation of temperature; and that the following season, the same plants, experiencing the usual gradations of temperature, will furnish an abundant harvest of grain. This proceeding has been successfully adopted with rye, by a member of an agricultural society at Valenciennes; he last year sowed the rye, and towards the close obtained two cuttings of green fodder; this year the same plants flourished so well, that they had attained a height of seven feet a month before the usual time of harvest.

Great variation of Temperature borne by the Human Body.

At the Hot-Baths of Kukurli, at Broussa, in Bithynia, (the natural temperature of which is as high as 183°... 189½° Fahr.,) the Duke of Ragusa states (in the notice of his Travels in the East, lately communicated to the *Académie des Sciences* by M. Arago,) that he saw a Turk remain for a *long time* in a water-bath, whose temperature *was 165°* Fahr. This fact, which *appears incredible, the most* positive manner; "for," says he, "I saw the fact with my own eyes. Dr. Jeng, an Austrian physician, saw it also, and remarked at the time what a very extraordinary circumstance it was. I therefore give it as perfectly correct."

Our readers may remember that the late Sir J. Banks and Sir C. Blagden, remained in an oven with a shoulder of mutton till this was thoroughly baked.

The power of enduring great, though not abrupt changes of temperature, has been put in a striking point of view during the late Polar expeditions. In the winter of 1833-4, Captain Back and his party, while residing at Fort Reliance, on the Great Slave Lake, were exposed to an average temperature of −33° (65° below the freezing point,) during the whole month of January, and on the 17th, the thermometer was as low as −70°, (102°−fr. pt.) A six ounce bottle of sulphuric ether was laid on the snow; in fifteen minutes the interior upper part of the phial was coated with ice, and the ether became viscid and opaque; a similar bottle of nitric ether indicated the same effects in two hours. Pyroligneous acid froze in thirty minutes at a temperature of −57° (89°−fr. pt.); a surface of four inches of mercury, exposed in a common saucer, became solid in two hours.

"On the 4th of February, the temperature was −60° (92°−fr. pt.), and there being at the same time a fresh breeze, was nearly insupportable: with eight large logs on the fire, in a small room, I could not get the thermometer higher than + 12° (20°−fr. pt.,) and ink and paint froze on a table placed as near the fire as I could bear the heat"—"the skin of the hands cracked and opened into unsightly gashes, which we were obliged to anoint with grease. On one occasion, after washing my face within three feet of the fire, my hair was absolutely clotted with ice before I had time to dry it."

On the 25th of January, the thermometer was at −18° (50°−fr. pt.,) and on the 26th it had risen to + 22°, (10°−fr. pt.,) while on the following day, it fell again to − 49° (81°−fr. pt.,) thus, in the course of twenty-four hours, an inequality of temperature of 71° had been experienced; on the 8th of February, a difference of 38° took place in the same time, and on the 21st, of 43°. On the 3rd of April the thermometer was at + 51°, (19° + fr. pt.,) and on the 28th of May, it stood at + 81° (49° + fr. pt.,) and yet on the 1st of that month it had been down at − 11° (43° − fr. pt.) When the party set out on their expedition in the month of June, they travelled over the *ice* on the lakes and rivers, and yet were compelled to journey by night, because the oppressive sultry heat during the day was so great, as to fatigue the men, and knock up the dogs; and yet the thermometer frequently fell below the freezing-point, during the night.

New Link between the Great Divisions of Organized Beings.

M. Guerin has detected under each of the first rings of the abdomen, in the *Machilus polypoda*, of the order *Thysanoura*, orders of respiration analogous to those found under the abdomen of many of the *Crustacea;* in all other respects the insect perfectly agrees with the characters of its class.

Vapours of Saline Solutions and of Water, same in Temperature.

It is generally admitted, that the aqueous vapour which is disengaged from saline solutions in ebullition, has exactly the same temperature as the superior stratum of the solution, and that thus the vapour possesses, but in this case only, an elasticity equivalent to the atmospheric pressure, a less, consequently, than it would have attained at its maximum of density, when its temperature would be much higher than 212°. It appears also natural, that each rising bubble of vapour should acquire immediately the temperature of the fluid which surrounds it on every side, and should continue at the same time to be subject to a certain expansion until its elasticity becomes equal to the atmospheric pressure. This also perfectly agrees with what happens, when vapour is disengaged from saline solutions at a low temperature, by the effect of evaporation; the vapours of such solutions being always less elastic than that of pure water, at the same temperature.

"All *physiciens* appear to hold on the temperature of vapour, the opinions we have just expressed," says M. Rudberg, *and he quotes to support this assertion,* several passages from the works of MM. Biot, Gay-Lussac, Pouillet, &c.; he then adds, "but notwithstanding these authorities, such a view of the subject is incorrect, in fact, *the temperature of the vapour which rises from a saline solution in ebullition, is independent of the nature and of the quantity of the salt dissolved; it is, under the same barometric prèssure, absolutely the same as that of the vapour which rises from pure water.*"

M. Rudberg had been led to make some rigid inquiries into the subject, after having found that water put into ebullition, either in vessels of glass or of metal, always gives out, under the same atmospheric pressure, vapour of precisely the same temperature, although the water itself, when it boils, be, as M. Gay-Lussac had already observed, actually hotter when it is in the first kind of vessels than when in the second. As in this case the difference of the temperatures of boiling water is produced by an unequal adhesion of the water to the interior surfaces of the vessels, it would appear more than probable that, in the same manner, the attraction of the salt by the water raises the *solution* only to a temperature superior to 212°, without exercising any analogous influence on that of the *vapour.* The experiments of M. Rudberg have fully confirmed this conjecture.

He began by constructing an apparatus which could not be affected by the influence of any external disturbing circumstances; he then made his experiments on different days, and under different atmospheric pressures. His intention being principally to ascertain by these means, if the temperature would follow a corresponding march to that of the barometer, because, in this case, it would be the best proof that the cause, through which the vapour possesses a temperature inferior to that of the saline solution, and exactly the same as that of the vapour of distilled water under the same pressure, ought not to have been sought in the refrigerating action of the air, but in the phenomenon itself of the formation of vapour.

During these series of experiments, M. Rudberg examined many times the temperature of the vapour of distilled water, principally with the intention of ascertaining if, by the effect of continued heat, the point of ebullition of the thermometer did not undergo a small change of place. This point was raised, in fact, 0.054 Fahr., and subsequently remained at this height.

The liquids which were submitted to experiment were solutions of muriate of lime, of neutralized carbonate of potash, of saltpetre, of common salt, and of sulphate of zinc. At the commencement of each series of experiments, the solutions were so concentrated that there remained, at the temperature of the chamber, much salt undissolved: by boiling the solutions again from time to time, they were gradually still more concentrated. The conclusion drawn from the experiments was, that *the vapour which rises from a saline solution in ebullition has precisely the same temperature as that which is disengaged from distilled water under the same atmospheric pressure, whatever be the number of degrees which the temperature of the solution may be still more elevated by reason of the quantity and the nature of the salt dissolved.*

Such is the first consequence, to which the results of these experiments have led M. Rudberg, in the most satisfactory manner. Other important ones seem also to flow from them, but these have not yet been established beyond doubt by direct experiment.

If we now consider the formation of vapours in saline solutions by evaporation, the experiments of MM. Dalton, Gay-Lussac, and Prinsep, have established that the vapour of a saline solution has an elasticity much less than that of pure water, when the two liquids have the same temperature. It therefore follows, inversely, that for the same elasticity, the vapour of a saline solution is hotter than that of pure water. It results likewise from experiment, that this difference of temperature increases with the quantity of salt dissolved, and that it also varies according to the nature of the salt. From these two results it clearly follows that *between the temperature of vapour and its elasticity, the relation is sensibly different, according as the vapour is produced by the ebullition of a saline solution or by evaporation from its surface.*

The cause of this difference, and that of the uniformity of the temperature of vapour formed by water and by saline solutions, under the same atmospheric pressure, can only be finally ascertained by a more exact examination of the formation of vapours in saline solutions by evaporation.

New Term in Physics.

It is now well ascertained that certain bodies, organic or inorganic, simple or compound, when in contact, mutually act on each other in a manner distinct from simple chemical agency, and that compounds are the result, the elements of which are not immediately traceable in the original substances. In the organic world, the effects of this unknown power are numerous and well known in the various animal and vegetable secretions, and the branch of science which analyzes the products and their apparent origin has been hence called *organic chemistry*. In our present state of ignorance of the subject, M. Berzelius proposes to give the name *catalytic* to this unknown agency, and *catalysis* to the decomposition of the bodies resulting from it. The apparent secretion (if we may apply the term here) of minerals in veins of rocks, not apparently containing the immediate elements of these depositions, is the most remarkable example of the effects of catalytic force in the inorganic world; the recent researches of Mr. Fox, as explained by him at the Bristol British Association meeting, seem to promise to throw some additional light on the subject.

Comparison of distant simultaneous Vibrations of the Magnetic Needle.

Herr. Gauss, Astronomical Professor in the University of Göttingen, has delineated upon a chart, the variations of the magnetic needle, as they were observed, on the 1st of April, 1835, in the cities of Copenhagen, Altona, Göttingen, Leipsic, and Rome*. The vibrations are represented by circular arcs drawn to the same scale. These arcs were found to be the longest at Copenhagen, and the shortest at Rome. The result is peculiarly deserving of attention, as the day of observation happened to be one on which an eruption of Vesuvius occurred. The comparatively feeble oscillations at Rome at such a time, would seem to contradict the opinion grounded upon some other observations,—that this kind of phenomenon has an influence on the magnetic needle.

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* Selected, it is presumed, from their differing little in longitude.

sphorescence of Decayed Wood.

nvestigations of M. Dessaignes on
ubject have been recently con-
l by experiments made by M.
. The results are, first, that
ent kinds of decayed wood placed
favourable conditions, exposed to
pheric air and moisture, are capa-
shining in the dark; secondly,
he emission of light is at a maxi-
in oxygen, and is destroyed by
gen, azote, and carbonic acid;
y, that the wood ceases to be
ous when placed in vacuo, which
s that the phenomenon is only one of
combustion. It is also ascertained
a temperature of from 50° to 55°
no means essential to it, as was
ved by M. Dessaignes, at least
when the combustion has com-
ed; on the contrary, the phospho-
nce is not perceptibly diminished
he temperature of ice, and if the
l containing the decayed wood be
ged in a refrigerating mixture of
or five degrees minus, the lu-
usness continues for upwards of
our, and that heat does not appear
ugment it. M. Florio also ascer-
ed that if decayed wood, the phos-
escence of which was maintained
moistening with water, were wetted
alcohol, the light was extinguished
few minutes, while on the other
l, it continued more than a day if
ere applied in the same manner.
has also found that this phosphoric
does not present any trace of
rization or of electricity, and, finally,
simple means of measuring its in-
ity, he arranged three mirrors, and
d that the light received by the
l, after reflections from the first
second, was still perceptible in a
h; and also that the phosphores-
e was perceptible through an enve-
of six sheets of thin paper in which
ood was enclosed.

On Veratria.

Veratria now ranks among the most
ary ingredients in Materia Medica,
ust naturally be of great interest to
pharmaceutical and medical world,
tain so valuable a substance in its
ect purity, that is, very white, with-
eing adulterated with foreign ar-
s; the author *of these lines* take
opportunity of comparing the
ds hitherto pursued for the preparation, with the one lately recommended by the pharmaceutist Simon of Berlin.

Veratria was discovered in 1819, by Pelletier and Caventou, and at the same time by Meissner, in several plants of the genus Veratrum, and particularly in the root of *Veratrum album*, or white hellebore, and in the seed of *Veratrum sabadilla*, sabadilla seed.

The sabadilla seed was treated with sulphuric ether, which dissolved a volatile crystallizable acid, and a fatty and other substances; the residuum treated with boiling alcohol, a deep brown colouring-matter is obtained, which is filtered off and evaporated to the consistency of an extract. Cold water will now dissolve this extract, except a small quantity of fatty matter which is filtered off; the solution, however, has to be concentrated by evaporation, and filtered again, and then precipitated with sugar of lead, which yields a copious yellow precipitate, and an almost colourless liquor, which, after having passed some sulphuretted hydrogen, for removing any excess of lead, and filtered and evaporated again, is treated with magnesia. This precipitate, when dissolved in boiling alcohol, which is afterwards to be distilled off again, yields a pulverulent substance, the veratria, which is yellowish, but which may be whitened by repeated treatments with alcohol and precipitations of water.

Couerbe's method is to make an alcoholic extract from which alcohol is distilled off, and this brownish-red extract is now boiled with water acidulated with sulphuric acid, until a mineral alkali does not indicate any precipitate; by adding now a solution of potassa or ammonia, the base or veratria is precipitated in its yet impure state.

For obtaining it still purer, it is dissolved in very diluted sulphuric acid, and to the sulphate of veratria so obtained, are added some drops of nitre, acid, and the liquor is decomposed by potassa dissolved, and we obtain the alkaline matter, which is washed with cold water, and redissolved again with boiling alcohol, &c. Simon's method, however, as described in the Berlin Annals is very simple; the seed is treated with boiling alcohol, which is distilled off afterwards, and the extract boiled with water acidulated with sulphuric acid, until subcarbonate of soda will no more produce a precipitate; the

is set
n period th
d and filtered
precipitated by su
so as to leave the fluid
the kettle over the fire,
h will at once be removed;
it begins to boil, veratria coagu-
together, and may easily be re-
d. It is washed out with water
discoloured in the following man-
After having dissolved it in boil-
g alcohol, add then animal charcoal,
nd, after agitating for some time, filter
the fluid, which will at last become quite
clear; evaporate the spirits of wine over
a sand-bath, and the remaining mass in
a porcelain dish by means of water-
vapours. It is obvious now, in what an
improved manner veratria may be ob-
tained by this last process; according
to the first, when the veratria was filtered
off, and washed out with water, and
then redissolved again, concentrated
and precipitated again with soda, the
alkaloid was separated by pressing it
between blotting-paper, and must natu-
rally suffer a great loss in the product;
whereas, in the latter, the alkaloid is
separated from its acid solution, and it
runs from itself by means of heat in
the basic fluid to its proper substance.
The product by the former process was
forty grains from the ounce of clear
seed, and that obtained by the last pro-
cess is fifty-four grains, which is 33 per
cent. more.—*Silliman's Journal.*

Migration of North American Birds.

By the Rev. J. BACHMAN.

FROM a variety of accurate experiments
which have been made at different pe-
riods, it appears that the hawk, the wild
pigeon, (*Columba migratoria,*) and
al species of wild ducks, fly at the
mile in a minute and a half;
ate of forty miles an hour,
d eighty between the
the sun, and nine
in twenty-four
irds to pass
distant
day,

ki
one of
Fontainble
day at Malta,
ring which she be
the rate of a mile in
would be one thousand
and forty miles in twenty-four
That many birds continue the
grations by night as well as by day, and
are thus enabled to make an additional
progress, may be easily ascertained from
their notes, which, in Autumn and
Spring, the seasons of their migratio
we often hear by night. The cries
geese, cranes, and some species of
birds are distinctly heard, and oth
silently. Wild pigeons are fre
seen, at early dawn, in the hi
mosphere. They fly higher
than by day, and thus expe
inconvenience from dark
great Hooping Crane s
pauses in his migrations
middle States. I have h
notes as he was passing
mountains of the Al
was always too high
naked eye. This b
wing from his usu
the south, ascends i
of the air, and s
arrives at his
near the polar r

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Organic Origin of Tripoli Stone.

M. Ehrenberg has recently made the remarkable discovery that the siliceous rocks, apparently homogenous, friable and even laminated, and which are known by the name of Tripolies, (*polier schiefer* of Werner,) are entirely formed of distinct remains, or rather skeletons of infusorial animals of the *baccilarian* order, and of the genera *cocconema, synedra, gaillonella*, &c. These remains, which have perfectly preserved the form of the silicified bodies of these infusoriæ, may be seen with the greatest distinctness in a microscope, and may be readily compared with the living species which have been observed and accurately drawn by M. Ehrenberg. In many cases there is no appreciable difference; the species are discriminated by the form, or more distinctly, by the number of the transversal septa which divide their minute bodies; and M. Ehrenberg, who has succeeded in counting them, has found the same number of these septa in the fossile and in the living species. His investigations have been made on the tripolies of Bilin, in Bohemia, of Santa-Fiora, in Tuscany, and on those of Ile-de-France and Francisbad. It is only necessary to take a sample of one of these tripolies, that of Bilin for instance, to grate a little on a plate of glass, to temper the dust with a drop of water, and then to see by means of a good microscope, thousands, or rather millions of remains of these animalculæ; the greater number of the species are fresh-water, but there are also marine, especially in the tripoli of Ile-de-France. M. Dujardin endeavoured to verify this singular discovery; his attempts to do so by means of the tripolies of commerce, and of many collections, were totally fruitless; but fortunately, at the *Ecole des Mines*, he met with the genuine Bilin tripoli. M. Dufrénoy pointed out to him the difference between this pseudo-tripoli, which is a fresh-water depôt formed in recent geological æras, and tripolies, properly so called, which are either volcanic rocks, or carbonaceous schistus of a very ancient formation, modified by igneous agency, as may be observed in that of Poligni, near to Rennes. In these tripolies, in that of Venice, which is yellowish, and whose origin is not known, as *well as in many others*, spe*cimens, of which are to* be seen in the *Ecole des Mines* or in the *Jardin des Plantes*, nothing is to be distinguished but fine equal-sized grains of silica. The stone from Bilin is of a clear grayish-yellow, laminated, presenting all the appearance of a very modern deposit, and entirely composed of organic remains; these are cylindrical tubes formed by articulated rings, which with numbers of detached rings are found simply aggregated together without any cement or interposed matter; the rings are of various magnitudes, from the $\frac{4}{1000000}$ to the $\frac{6}{1000000}$ of an inch in diameter, their average altitude being about half their diameter; they may be discerned by means of a single lens, magnifying 70 or 80 times. These fossils are evidently organic remains, but in *form* they have no resemblance to the *baccilaria*, which are prismatic. From what M. Dufrénoy told him concerning the Santa Fiora tripoli, which is also a recent deposit, M. Dujardin conceives that analogous fossils will be discovered in other siliceous friable deposits belonging to the tertiary formations; but nothing of the kind is perceivable, even in the pulverulent siliceous deposit accompanying the Menilites of the Paris basin.

Chalk taken from certain parts of the cretaceous formation, has been found to consist of the *débris* of minute corals, foraminifera, and valves of a small entomostraceous animal resembling the *cytherina*. It is probable, therefore, that organic agency has been more extensively employed in the formation of the crust of the globe than has hitherto been supposed; the last-mentioned fact giving additional support to the hypothesis that lime is a product of organic origin. We hope that the naturalists of our own country will hasten to ascertain and confirm the truth of this singular discovery.

Hypotheses respecting Aurora Borealis.

Our readers are aware that at the late meeting at Bristol, Mr. Herapath described the appearances presented by a remarkable Aurora he witnessed in November last, from which he came to the conclusion that that phenomenon is merely electricity passing off from a charged cloud, in the act of dissolving in air that can take its water, but not its electrical fluid, which consequently becomes visible.

Professor Joslin. of Schenectady, U.S. has recently published a paper on the subject of Aurora, and, from a number of observations, comes to the conclusions, That the temperature of the air is falling, and the atmospheric pressure increasing, on the day in which an Aurora appears; that, generally, after an Aurora, the atmospheric pressure decreases, temperature rises, and water falls as snow or rain within two days; that the air at the earth's surface, if not saturated with moisture at the time of an Aurora, is much nearer than usual to that point.

It will be easily perceived that these results are conformable to Mr. Herapath's hypothesis. Professor Joslin, however, proposes another equally deserving of attention,—*that Aurora is the result of crystallization of the vapours of the atmosphere.* The following are the principal steps in the argument.

Light is given out during the process of congelation, and since it appears that the state of the atmosphere is favourable to the deposition of moisture, and to its crystallization from the decreasing temperature, it is hence *possible,* if not probable, that the light is the result of this action. If the molecules of crystals have a peculiar kind of polarity which governs them in their arrangement, they *may* be also affected by the magnetism of the earth, and thus would be produced the columns which Aurora usually presents, their fluctuations arising from the unsteady nature of the atmosphere and the continual reproduction of light from the crystallization of new quantities of vapour.

It has also been observed, that Auroras are most common during the months of November and December, and that when they consist merely of a bank of light, like the dawn, the magnetic needle is but slightly affected; but that when *beams* or *streamers* are formed, the disturbance of the needle seems to indicate a more active magnetic energy.

There is no contradiction between the hypotheses of these two gentlemen, and as all theories are only regarded by true followers of inductive science, as guides in the collection of new facts by which the real explanation of a phenomenon may, in time, be arrived at, *meteorologists* will have new incen*tives to extended* and accurate obser*vations.*

Convenient numerical Expression of the Welfare, &c., of a Nation.

M. Dupin, the present President of the *Académie des Sciences,* in his "Researches on the Influence of the Price of Corn on the population of France," read in May and June last before the Academy, has come to the conclusion, that it is untrue to assert that the abundance or scarcity of the means of subsistence, acts as the principal and predominant cause, in the amount either of births or of deaths in the French nation, and that "the years in which the greatest numbers of marriages take place, are not those in which the prices of provisions are the lowest."

He considers it as demonstrated, that "the fortuitous causes brought into action by intemperate seasons, commercial fluctuations, and the vicissitudes of human affairs, produce at the present time much greater inequalities in deaths, marriages, and births, than the extremes of abundance or scarcity.

"It is not, then, necessary or useful," he continues, "for Frenchmen to extol the desolating doctrines of Malthus, which deprecate the increase of the human race by the less opulent classes." And he rejoices "that Science is able to demonstrate that the speculative and systematic theories which freeze up the human heart and wound the natural feelings, are contradicted by facts, and have no foundation in experience."

He then proceeds to take advantage of the newly-discovered and welcome truth in the following manner:—

"The minute variations in the annual amount of births, marriages, and deaths, even when great alterations have taken place in the price of corn, have led me to seek for a function of these three social elements, which should exhibit these variations far more sensibly, by correcting, through the means of each other, the irregularities produced by the numerous unforeseen, accidental, and transitory causes.

"Every general cause of public prosperity multiplies, on the one hand, the births and marriages, and diminishes the deaths on the other.

"If we suppose a population so situated as to remain in the same social and physical circumstances, and which should at once be doubled, tripled, quadrupled, &c., the births, marriages and deaths, would be similarly doubled, uadrupled, &c.; consequently, ortion of the births and mar- the deaths would remain con- hatever might be the increase pulation.

ve taken the mean of the two proportions:

$$\frac{\text{Births}}{\text{Deaths.}} \quad \frac{\text{Marriages}}{\text{Deaths.}}$$

ave called it the *Function of*

Function is, as has been seen, lent of the total number of the nts. This, in the existing state s, offers a great advantage; for is, that up to the present day, us of the whole [French] popula- at all times been imperfect, but isters of the State have given, at accuracy, the annual number irths, marriages, and deaths.

very evident variations which ction of national Vitality under- the end of a certain number of re the mathematical expression sure demonstration of the great s which have affected the welfare eople.

the following Table are given e of Wheat, and the Function onsideration, obtained as above, year 1817 . . . . . 1830. The &c., are arranged into groups, fifth and sixth columns con- means of each group.

rative Table of the Prices of at, and the Functions of Vitality, fteen years, arranged according he Prices, beginning with the est.

rice of Wheat er Hectolitre.	Function of Vitality.	Sum.	Mean Function.	Mean Price.
r. cent.				fr. cent.
36 16	0·5788	1·1875	0·5937	30 40½
24 65	0·6087			
22 64	0·5989	2·4368	0·6092	22 48
22 59	0·6107			
22 32	0·6443			
22 03	0·5829			
19 13	0·5823	3·7007	0·6168	18 05
18 42	0·5777			
18 21	0·6220			
17 79	0·6128			
17 52	0·6838			
16 22	0·6221			
15 85	0·5890	1·8290	0·6097	15 69
15 74	0·6080			
15 49	0·6310			

first group embraces a period of *dearness, the mean of the prices is 30 fr. 40½ c.,* and the mean value of the Functions of Vitality is 0.5937.

"The second, a period of high prices; the mean of the prices is 22 fr. 48 c., and that of the Functions of Vitality, 0.6092.

"The third, a period of intermediate, or moderate prices; mean price, 18 fr. 05 c.; mean of the Functions of Vitality, 0.6168.

"The fourth, a period of the lowest prices; mean price, 15 fr. 69 c., and that of the Functions, 0.6097.

"The Function of Vitality of the fourth, or lowest-price period is, as may be observed, less than that of the third, or moderate-price period.

"It would, therefore, appear to be most favourable to the welfare of the nation in general, that the price of corn should fluctuate among the intermediate prices of 1 fr. above, and of 2 fr. below, 18 fr. per hectolitre of wheat.

"Before a further extension of the conclusions which may be drawn from similar considerations, it will be necessary to accumulate observations, and to proceed with great circumspection, so that there may be no temptation to hazard consequences, which futurity and facts more carefully collected and established, may not confirm.

"It would be desirable that the value of this Function should be inserted every year in the *Annuaire du Bureau des Longitudes.* It might be placed in the Summary of the Population-Tables which have been given—1817 . . . 1835.

"This new element would immediately attract attention to the degree of welfare or distress enjoyed by the population of France in each of the years so furnished.

"Statesmen, statisticians, and medical men, each in his own peculiar sphere, might then study the causes of the variations indicated by this element, and by this means, much valuable assistance would be afforded to such as may attempt to appreciate the causes which are favourable or otherwise to the amelioration of the condition of man.

"Fifty years ago, the mean value of the Function of Vitality, calculated upon an average of fourteen years, amounted to 0.5403.

"This same Function, calculated on an average of fifteen years, from 1817 to 1831, gives a mean of 0.6103. The superiority of the latter number is the expression of the immense advance in the welfare of the French people in the course of the last half-century.

"It is greatly to be wished that the same Function should be carefully calculated for other countries, in order that their welfare and prosperity might be accurately compared by one and the same scale."

Visible Sparks from the Torpedo.

No one up to the present time has yet perceived the electric spark in experiments made on the torpedo. M. de Humboldt, even in the native country of the gymnotus, did not succeed in observing it. Walsh, unsuccessful in numerous attempts to obtain it from the torpedo, succeeded, in August, 1776, in rendering the spark visible, in operating with a gymnotus. Fahlberg, Ingenhousz, and others, have stated, that they have observed, at times, a spark during the discharge of the electrical eel of Surinam.

MM. Linari and Matteucci, in some experiments upon torpedoes, conducted with great care, have arrived at the most successful results. A letter, addressed by the latter philosopher to M. Arago, contains some curious details on the subject.

The author, in the first place, describes the apparatus by means of which M. Linari was able to obtain from a single torpedo as many as ten sparks in succession, every one very visible and brilliant. As far as M. Linari could ascertain, neither the size, the age, nor the sex of the animal made the slightest difference in the production of the spark. A small torpedo, of four inches and a half in diameter, furnished him with a long train of very brilliant sparks. They were also obtained from a torpedo which had been kept three days out of the sea, but then the brine of his tub was obliged to be perpetually renewed. The decomposition of acidulated water, and the permanent magnetizing of steel needles, were constantly obtained by M. Linari.

M. Matteucci, after describing his own apparatus, which differs but little from that of M. Linari, states that he, in the first place, verified the results of the latter experimenter, and then succeeded in obtaining, with the current of the torpedo, all the phenomena of the ordinary electric current.

"I came finally," says M. Matteucci "to the most conclusive experiments that I could have desired for the developement of the electric current in the torpedo. These experiments are always extremely difficult, from the feeble vitality of the animal when often irritated, and from the extreme care necessary to preserve it alive when out of the sea.

"By means of a very sensible galvanometer, I found that the current in the torpedo is constantly in the direction from the back to the abdomen. The former may be considered as the positive, and the abdomen as the negative pole of its apparatus. The discharge acts in the same direction in the two organs which are on the sides of the torpedo, the current is also obtained precisely the same, when one of the poles of the galvanometer touches the lower part of the right organ, and the other the back of that on the left, or inversely. The deviation of the galvanometer is increased, if instead of directly touching the skin of the torpedo with the platina poles, they are brought in contact with slips of metal, previously placed on the two surfaces of the fish; but a continuous current is never produced, however much the organ may be compressed. The discharge is generally produced with certainty, if the animal is bent so as to render the abdomen slightly concave. If the skin with which an organ is covered be removed, the deviation diminishes in intensity, but always occurs again when the animal discharges. When it ceases to discharge, not the least trace of the electric current can be detected in any part of the organ. The deviation is also altogether suspended, when the two extremities of the galvanometer touch the back, or the abdomen, of the fish at the same instant. If of the three nervous bodies which run from the brain and penetrate an organ, two are cut away, the discharge still continues, but it immediately ceases on cutting away the middle one. The other organ, if left entire during these operations, continues to act without interruption."

Fluorine.

It is not commonly known in this country* that this simple substance has been obtained in its separate state. M. Baudrimont, in a dictionary of general physics, published in December, 1834, indicated its general properties in these terms: "Fluorine is a gaseous body of a deep brownish-yellow; its odour is similar to that of Chlorine, or to that of burnt sugar; *it does not act on glass;* it deprives indigo of its colour, and combines directly with gold." M. Baudrimont has recently obtained it by treating a mixture of fluoride of calcium and peroxide of manganese, with sulphuric acid in a glass bottle; the excess of one of the substances employed would produce either oxygen or hydrofluoric acid, and this last by its action on the glass would be changed into silicated-fluoric gas. His first method of extracting the fluorine, consisted in decomposing fluoboric gas by means of the deutoxide of lead heated to redness, and he obtained fluorine but less pure than subsequently.

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* See Vol. I., p. 294.

M. Pelouze has lately announced another mode; he decomposed fluoret of silver in water by means of chlorine,—the result, according to M. Baudrimont, ought to be a compound of hypochloric and hydrofluoric acids.

A great difference is deserving of remark between the fluorine obtained by these processes and that which might have been expected to be the base of fluoric or hydrofluoric acids, so well known for its energetic action on glass, and which appears to be more deleterious than other hydro-acids.

We may hope, therefore, soon to be better acquainted with fluorine than with fluoric-acid itself, which can neither be prepared nor preserved in glass vessels.

Railroad Acts to end of recent Session.

The following Railroad Acts have received the royal assent since our notice, p. 78; they complete the list for the recent Session of Parliament.

28th July.	30.	London and Blackwall Commercial Railway.
"	31.	Tremoutha Harbour and Railway.
Aug. 13.	32.	Edinburgh, Leith, and Newhaven Railway.
"	33.	Dublin and Drogheda Railway.

Parliament was prorogued on the 20th of August.

United States New Patents' Act.

The Bill* for the establishment of a Patent Office in the United States, and for placing this branch of the administration under the control of the legislature, on a simple and rational system, intelligible to all, has passed into a law. It received the fiat of the President ("Approved, Andrew Jackson,") on the 4th of July last,—the Sixtieth Anniversary of American Independence. The act is called an *Act to promote Useful Arts*, and we look with envy on the title. Nothing could be more in perfect harmony with the memorable day on which it obtained the American "Royal Assent," and though we admit that the passing of such an Act richly deserved an anniversary of its own, yet, taking it as it occurred, what could possibly be more peculiarly impressive than the contemplation of the chief magistrate of a great nation, on the anniversary of that nation's independence, giving validity to an Act to promote the useful arts. A more appropriate subject for an historical picture in the Washington Capitol can scarcely be imagined.

From such a scene, if we turn our eyes homeward, what humiliation awaits our national pride; here we see high official personages clutching exorbitant fees, and baffling every attempt made by our most enlightened and intelligent countrymen "to promote the useful arts." Here we see delay, and manœuvre, and deception, successfully deferring, year after year, a measure deeply interesting to our native talent, and universally important to the national manufactures. Is it possible to contemplate unruffled this contrast between "the Old and the New Country?" In this, a fee of a few dollars on each patent, without a farthing from the government, maintains an office, in which qualified persons are in attendance to advise in cases of doubt, and to examine, and register, and exhibit all specifications, in which are provided scientific books, periodical publications, models, and collections of machines, products, &c., all selected and preserved for the purpose of spreading accurate information on subjects connected with patents, patentees, and patent-rights. In our country, nearly £30,000, perhaps more, has been paid within the current year to *high public functionaries, who do nothing in the matter*, out of the pockets of inventors seeking security for their own discoveries;—what have they received in return? —*about* 300 *lumps of green wax!* not a "drachma" more; not a particle of information has been granted—not the slightest guarantee of the validity of the patent has been vouchsafed, and so little real knowledge has been purchased by this enormous outlay, that the instruments to which these lumps are appended, have been correctly defined by one intimately conversant with them, to be generally "traps for the unwary, sources of heart-breaking annoyance, and, in many cases, causes of utter ruin."

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* An Abstract of this Bill is given at p. 76 of the present volume.

The original bill of the United States is so little altered by the Act, and the abstract we gave of the former at p. 76, is so copious and correct, that we consider it unnecessary to reprint the new document. There are some additions, of which the following are the principal.

Commissioner has the privilege of franking all letters and packages by mail, relating to the business of the Patent-Office.

On a renewed application for a patent being refused, applicant may have examiners of his claim appointed.

Inventor not to lose his right to a patent, "by reason of his having previously taken out letters-patent therefor in a foreign country, and the same having been published at any time within six months, next preceding the filing of his specification."

Patent, on request, may date from filing of specification, if the same do not exceed six months from issuing of patent.

Specification, on request, may be preserved secret in the Patent-Office for a year, or until the necessary models be furnished; during which time applicant to be entitled to notice of interfering applications.

Caveat must set forth the design and characteristics of invention,—may be preserved secret in Patent-Office. Person filing caveat to file his specification, &c., within three months after receiving notice of interfering application.

Defendant in an action relying on previous invention, knowledge, or use of the thing patented, must give special notice of the name and residence of party possessing such prior knowledge.

Patentee believing himself inventor at time of application, not to lose patent on *account of invention* having been known or *used in a foreign* country, it not appearing *that it had* been patented or described in *any printed publication*.

Patent term may be extended seven years beyond the first term of fourteen, on application to commissioner, setting forth reasons, giving public notice of same, furnishing the Board appointed to judge with description of invention, profit and loss, &c., and obtaining a favourable decision of the said Board. Commissioner is then to extend the patent by a certificate of said decision. No grant of extension can be made if the first term be already expired.

Applications pending at time of passing Act, and on which duty is paid, to be proceeded with as though filed after the passing thereof.

We have extracted the Fees, and tabulated them for convenient reference, and also to exhibit more conspicuously the vexatious pre-eminence as to expense, which a "subject of the king of Great Britain" must hold, so long as his own government will not follow the example of other nations, and relieve or remove the oppressive burden.

Duties to be paid on Applications, &c., for Patents, and other Business relating thereto, under an Act of the United States, passed the 4th of July, 1836, entitled "An Act to promote useful Arts," &c.

		Dolls.	£	*s.*	*d.*
1.	Caveat, to be returned if Patent be subsequently taken	20...	4	10	0
2.	Application for Patent,—				
	(*a*) If by a Citizen, or an Alien-resident of a year, having made oath of intention to become Citizen	40...	9	0	0
	(*b*) If by a Subject of the king of Great Britain	500...	112	10	0!
	(*c*) By all other Persons	300...	67	10	0
3.	Request for Examination of Application	25...	5	12	6
4.	Addition of new Improvement to an original Specification	15...	3	7	6
5.	Extension of Patent	40...	9	0	0
6.	Register of Transfer of Patent Right	3...	0	13	6
7.	Surrender of an invalid Patent for Renewal	15...	3	7	6
8.	Attested Copy of Specification, per 100 Words ....10 *Cents* ..		0	0	5¼
9.	Copies of Drawings, "reasonable expense of making them."				

An Examiner not to be paid, in any one case, more than 10 Dollars (£2. 5*s.*) for his services.

We must not be understood as giving our unqualified approbation to the whole of this New Patents Act of the United States, nor expressing an opinion that it is perfect; but in our extreme destitution it would be absurd to be fastidious, and we cordially congratulate our Trans-Atlantic brethren on their valuable acquisition, hoping, though against conviction, that a few years may wipe out one article, at least (viz., 2 b), from their Tariff.

Patent-Law Grievance. No. VII.

The penalties inflicted on the inventive genius of Britain during the present year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £33,000!

N.B. This sum has been paid in *ready money*, on taking the first steps, and as many of the inventors are poor men, (operatives,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

NEW PATENTS. 1836.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

Total, August...19.

September.

204. Robert Griffiths, Birmingham, *Warw.*, Machine-maker, and John Gold, of the same place, Glass-cutter; for improvements in machinery for grinding, smoothing, and polishing plate-glass, window-glass, marble, slate, and stone; and also glass vessels, and glass spangles, and drops. Sept. 1.—March 1.

205. John Pickersgill, Coleman-st., *Lond.*, Merchant; for improvements in preparing and in applying India-rubber (caoutchouc) to fabrics. Sept. 1.—March 1. *For. Comm.*

206. James Surrey, York-house, Battersea, *Surry*, Miller; for a new application of a principle by which mechanical power may be obtained or applied. Sept. 1.—Jan. 1.

207. William Bush, Wormwood-st., *Lond.*, Surveyor and Engineer; for improvements in the means of, and in the apparatus for, building and working under water, part of which improvements are applicable for other purposes. Sept. 3.—March 3.

208. Charles Farina, Clarendon-pl., Maida Vale, *Middx.*, Gent.; for an improved mashing apparatus. Sept. 15.—March 15.

209. William Hinckes Cox, Bedminster, Bristol, Tanner; for improvements in tanning hides and skins. Sept. 15.—March 15.

210. John Frederick William Hempel, of Oranienburg, *Prussia*, Clapham, *Surry*, Officer of Engineers, and Henry Blundell, Hull, *York.*, Paint and Colour Manufacturer; for an improved method of operating upon certain vegetable and animal substances in the process of manufacturing candles therefrom. Sept. 15—March 15. *For. Comm.*

211. Joshua Bates, Bishopsgate-st., *Lond.*, Merchant; for improved apparatus or machinery for making metal hinges. Sept. 15.—March 15. *For. Comm.*

212. Peter Ascanius Tealdi, late of Mondovi, *Piedmont*, Manchester, *Lanc.*, Merchant; for a new extract or vegetable acid obtained from substances not hitherto used for that purpose, which may be employed in various processes of manufacture, and in culinary or other useful purposes, together with the process of obtaining the same. Sept. 15.—March 15. *For. Comm.*

213. William Bates, Leicester, Fuller and Dresser; for improvements in the manufacture of reels for reeling cotton. Sept. 16.—March 16.

214. Moses Poole, Lincoln's-inn, *Middx.*, Gent.; for improvements in the description of public vehicles, called cabs. Sept. 21.—March 21. *For. Comm.*

215. Robert Jupe, Bond-st., *Middx.*, Cabinet-maker; for improvements in apparatus applicable to book and other shelves, Sept. 22.—March 22.

216. William Crofts, Radford, *Nott.*, Machine-maker; for certain improvements in machinery for making bobbin-net lace, also called twist-net or lace, part of which improvements are for the purpose of making figured or ornamental bobbin-net-lace, or ornamental twist-lace. Sept. 22.—March 22.

217. Henry Van Wart, Birmingham, *Warw.*, Gent., and Samuel Aspinall Goddard, of the same place, Merchant; for certain improvements in locomotive steam-engines and carriages, parts of which improvements are applicable to ordinary steam-engines and to other purposes. Sept. 22.—March 22.

218. John Smith, Halifax, *York.*, Dyer; for certain improvements in machinery for dressing worsted and other woven fabrics. Sept. 22.—March 22.

BRITISH ASSOCIATION.

BRISTOL SESSION.

As we by no means consider the appointments of officers to the British Association as merely honorary, or complimentary, we have thought it a duty to obtain a more correct list of them than it was possible to do before the publication of our last Number. We now insert it. The visiters of the several Sections may therefore see to whom the management of each was confided. If we could have extended the salutary regulation recently adopted in Parliamentary Committees, and thus have been enabled to report the actual attendance of each officer, and each committee-man, we would have cheerfully done so. We have been told that many members were never seen at the Committees to which they were appointed. To prevent those members from being either commended or blamed, who were not at Bristol at all, we have printed their names in italics.

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ERRATA.

Page 143, line 25, *for* less, *read* greater.
—— — 26, *for* greater, *read* less.
—— — 28, *for* 236, *read* 210.

METEOROLOGICAL JOURNAL FOR AUGUST, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month	Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer.		Daily Temp.	Solar Var.	Rad.	Clouds.		Wind.		Direction of wind		Luna-tion.	WEATHER, &c.
					Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Mon. 1	30·294	65°	30·152	67°	50°9	70°0	60°4	19°1		8	6	2	3	S.W.	W.S.W.		Clouds and wind; evening fine.
Tues. 2	30·158	65	30·200	67	48·6	68·2	58·4	19·6		4	2	2	1	W.b N.	W.S.		Fair; *Cumuli ;* very fine day.
Wed. 3	30·122	67	29·893	71	50·5	77·5	64·2	27·0		3	3	2·3	2	S.S.E.	S.E.		Ditto *Cirrus, cirro-cumuli*, in groups.
Thurs. 4	29·975	64	29·990	64	54·2	72·8	63·5	18·6		10	4	1	1	S.W.	S.	☾	Light rain; clear at night.
Friday, 5	30·115	63	30·135	64	57·0	73·6	65·3	16·6		10	6	2	2	N.N.E.	N.E.		Rainy; evening cloudy.
Satur. 6	30·230	62	30·260	63	55·1	68·0	61·6	12·9		10	7	2	2	N.	N.		Cloudy till 4 P.M.; evening clear.
SUN. 7	30·170	66	30·230	66	51·5	72·0	61·7	20·5		8	6	2	1	N.	N.		Cloudy.
Mon. 8	30·300	61	30·315	62	51·5	72·0	61·7	20·5		1	1	1	1	N.N.E.	N.E.		Mostly clear; lightning at night.
Tues. 9	30·345	62	30·320	63	50·0	70·0	60·0	20·0		5	3	2	2	E.	N.E.		Cloudy till noon; fine
Wed. 10	30·305	62	30·295	63	46·5	73·0	59·7	26·5		4	3	2	2	N.	N.		Fine and warm.
Thurs. 11	30·430	62	30·432	62	53·0	69·4	61·2	16·4		5	8	2	2	N.E.	N.E.		Fair.
Friday, 12	30·475	64	30·445	64	56·2	68·7	62·4	12·5		8	2	2	2	N.E.	N.E.	●	Morning cloudy; evening fine.
Satur. 13	30·330	61	30·270	64	48·0	73·1	60·5	25·1		2	3	2	2	N.E.	N.E.		Fine; evening, lightning.
SUN. 14	29·970	65	29·935	65	55·0	74·5	64·7	19·5		10	6	0	0	calm.			Thunder-storm at 8 A.M., with rain till noon; fine [afternoon and night.
Mon. 15	30·085	62	30·105	63	57·0	73·0	65·0	16·0		9	7	1	1	S.S.W.	S.W.		Hazy and cloudy.
Tues. 16	30·325	60	30·280	61	54·5	70·7	62·7	16·2		10	5	1	2	N.E.	N.E.		Cloudy A.M.; afternoon fine.
Wed. 17	30·230	66	30·255	68	57·1	73·2	65·2	16·1		4	6	2	2	S.W.	S.W.		Alternately clear and cloudy.
Thurs. 18	30·195	64	30·130	66	54·0	71·0	62·5	17·0		7	9	2	2	W.S.W.	W.		Squally with showers.
Friday, 19	30·278	66	30·282	67	51·5	66·5	59·0	15·0	50°	5	5	2	2	N.W.	N.W.	☽	Fine.
Satur. 20	30·065	66	29·894	67	46·4	63·0	54·7	16·6	46	10	8	2	1	S.S.W.	S.W.		Rain; *cirro-stratus* and *scud.*
SUN. 21	30·060	66	30·053	67	49·8	66·4	58·1	16·6	49	6	2	1	2	N.W.	W.		Fine.
Mon. 22	29·915	65	29·826	66	48·7	68·5	58·6	19·8	47	9	9	2	1	S.W.	S.W.		Overcast.
Tues. 23	29·756	66	29·802	66	54·9	68·0	61·5	13·1	54	10	10	1	2	N.	E.		Rain at midnight; afternoon and evening continued [rain.
Wed. 24	30·075	65	30·192	66	50·0	62·9	56.5	12·9	50	10	5	2·3	0	N.E.	N.E.		Rain till noon; afternoon fine.
Thurs. 25	30·292	64	30·240	67	42·9	70·0	56·5	27·1	42	5	8	1	1	E.	S.		Fine; P.M. overcast; *cirro-cumuli.*
Friday, 26	30·100	64	30·091	65	52·6	68·5	60·5	15·9	50	10	5	1	1	W.S.W.	W.	○	Rain till 2 P.M.; afternoon fine, with *cumulus.*
Satur. 27	30·085	64	30.038	66	48·4	70·3	59·3	22·1	47	8	4	2·3	2	W.S.W.	W.S.W.		*Scud;* much wind; very black night.
SUN. 28	30·092	64	30·080	66	56·0	65·5	60·7	9·5	55	10	5	0	1	E.	N.		Rain with fog till noon; fine evening and night.
Mon. 29	29·260	65	30·250	66	45·5	63·0	54·3	17·5	42	3	6	1	1	W.	W.		Very fine day; evening overcast.
Tues. 30	30·316	65	30·281	67	47·7	70·1	58·9	22·4	46	5	8	1	2	W.	W.S.W.		Mostly cloudy.
Wed. 31	30·205	66	30·150	68	53·8	70·5	62·1	16·7	52	2	0	2	2	S.S.W.	S.S.W.		Very fine; cloudless.
Mean	30·179	64	30·156	66	51·57	69·82	60·70	18·25									

Bar. Max. 30·475 in. on the 12th. || Ther. Max. 77·5° on the 3rd. || Lowest point of Rad. 42°, on the 25th. (Since the 19th.)
Bar. Min. 29·750 in. 22nd. || Ther. Min. 42·9° 25th. || Rain fallen 2·555 in.

DISCOURSE ON SOME ADVANCES WHICH THE MATHEMATICAL SCIENCES HAVE MADE IN FRANCE

SINCE THE YEAR 1830;

DELIVERED BEFORE THE ACADÉMIE ROYALE DES SCIENCES, PARIS, AT THE ANNUAL PUBLIC MEETING ON THE 28TH DECEMBER, 1835.

BY THE BARON CHARLES DUPIN, PRESIDENT OF THE ACADEMY.

I REQUEST permission to point out a few of the mathematical conquests accomplished since 1830. It will be seen that, in this period, though so short and so deeply agitated, the sciences, prosecuting their march with tranquil courage, have not disregarded the reputation of France.

At the very moment from which I propose to commence, the Academy lost one of its most illustrious members. M. Fourier was of that class of minds which, profoundly meditative, and more anxious to multiply their discoveries than to accumulate fame, preserved a long silence over works, any one of which was amply sufficient to have acquired renown for its author. Such is *La Résolution générale des Equations numériques*, the foundations of which he laid at the age of eighteen, and had developed in some lectures, delivered before he set out with the Egyptian expedition. This work was found in manuscript among his papers after his death. In his modes of solution are combined the advantages of rigorous exactness, and a ready and easy application,—a twofold merit that Lagrange himself did not attain in his excellent work on the same subject. M. Fourier sometimes transforms and improves methods distinguished by the names of the great Newton, of Lagrange, and of Daniel Bernouilli; at others, he becomes himself an inventor; as in his Theory of Inequalities, a theory entirely original, and of which he exhibits the fertility. The work in which he has displayed such inventive genius has been published since his death, complete and correct, as all posthumous writings ought to be. M. Navier*, the friend, the pupil, and the worthy colleague of M. Fourier, has executed this task with a talent, a devotedness, an almost filial respect, honourable alike to the great geometrician and to himself.

The writings we have noticed possess, most decidedly, the characteristics of M. Fourier. Each theoretical discovery is illustrated by important applications, and each of these, impracticable before, is accomplished by methods of general analysis, invented by him expressly for the purpose. It is this double success which is so admirable in his *Théorie Mathématique de la Chaleur*, and in the methods of integration which characterize and enrich it.

In the person of M. Legendre, the exact sciences suffered another irreparable loss. At the age of seventy-eight, he carried still nearer to perfection, and reprinted, his Theory of Numbers; at eighty, he completed and published the third and last supplement to his *Fonctions Elliptiques;* a work in which immense calculation, and novel and profound research demonstrate the degree in which this veteran, illustrious by genius, and venerable by character, had retained his intellectual vigour. Rewarded by Napoleon for having, with Lagrange and Laplace, maintained, during the great reign, the mathematical supremacy of France; degraded, ten years later, for having refused to prostitute his academic suffrage, insolently demanded, as if it were a tribute, by monarchical power, and for having rung in the ears of his countrymen, the indignant cry of his insulted honour, M. Legendre bore his glorious adversity without a murmur; he consoled himself, amidst his fallen fortunes, by rendering new services to science, and, at last, having done all in human power to deserve unspotted glory, he interdicted, on his death-bed, the academic eulogy; but he cannot prevent our planting the remembrance of his career, and the example of his virtues, as a fruitful germ in the hearts of the neophytes of science.

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* *Recently deceased.*

Faithful to the system which admitted Huygens and Cassini, without regard to country, to her bosom, the Academy invited to the vacant seat of Legendre, a geometrician who was born in the country of Galileo. Our later volumes are enriched by his researches into the integration of linear equations having differences of the second and higher orders. M. Libri had published the first volume of his *Histoire des Mathématiques en Italie;* but at the moment of presenting it to the Institute, the whole edition, partaking the fate of the *Mécanique Céleste*, was destroyed in the vast conflagration which Paris still deplores*. The author supported his loss with the calm of a philosopher, and exemplified one of the principal precepts which his own work inculcated. In this History, he has elevated a monument to the philosophy of the sciences, exhibiting to youth, not the ordinary spectacle of genius, welcomed, honoured, rewarded, but of genius, attacked, persecuted, proscribed, as were Galileo, Dante, and Machiavelli. "This persevering struggle, this great intellectual drama," says M. Libri, "appears to me to contain high lessons of morality, particularly useful at a time when discouragement and suicide follow so close upon the least disappointment occurring to the young." It is delightful to see the most austere of sciences thus tendering her masculine remedies to youth empoisoned by a deleterious literature; and offering to noble minds, in the hope of attaching them to life, the sublime ambition of deserving, by our services and our fame, the ingratitude and the insult of our cotemporaries.

The transcendental analysis continues to be cultivated by the scientific descendant of Laplace and Lagrange. In addition, M. Poisson is extending a field of inquiry, which has been approached, in turns, by two geometricians, Legendre and M. Biot, among the French, and MM. Ivory and Gauss, in foreign countries; viz., *the attraction of homogeneous ellipsoids, whatever may be the position of the point attracted.*

He is also examining anew the motion of the moon round the earth, a subject which has been successively illustrated by the greatest mathematicians, from Newton to Laplace, and since them, by MM. Plana, Carlini, and Damoiseau. In order to solve the great difficulties which attend this inquiry, M. Poisson has sought the means of solution in the method, at once general and fertile, of the variations of arbitrary constants, a method which he completed about the time that Lagrange, *septuagénaire*, erected this last monument of his genius.

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* An immense fire broke out in the quarter in which nearly all the principal bookbinders resided. The literary loss was immense.

Grappling even with the calculus of variations, the great analytical discovery of the eighteenth century, but in which still remained as a desideratum the solution of a general and very difficult case, viz., the variation of double integrals, taking the terms of this variation which correspond to the two limits as perfectly complete, he fills up this chasm, and furnishes new means of applying this admirable calculus to mechanical and physical questions.

The general theory of the motions of a solid body has become the object of the simultaneous studies of two able geometricians, the one, powerful in the resources of analysis, the other, in those of geometry. The first, whose labours we have just noticed, maintained his previous reputation by demonstrating the fertility of his favourite method. The second, M. Poinsot, the inventor of the new regular polyhedrons, and the creator of the theory of couples which has so changed the face of statics and mechanics, undertook to treat, by geometry alone, the transcendental questions of the double motion of bodies, by translation and rotation. If we imagine the momentary axes of rotation, and the principal axes of a body to be represented by apparent right lines, and that upon these right lines there are distinct points marking the position of the centre of gravity of the body, we can form orbs indicative of the translation, and surfaces indicative of the rotation. Now, by the ingenious reasonings and lucid theory of the author, the geometrical properties of these orbs and surfaces which are correlative to those of the body and of the forces acting upon it, are rendered, as it were, visible to the eye, and palpable to the imagination; they offer a series of theorems as novel as unexpected; they introduce to the mind one of the most exalted pleasures that our intellect can experience, when it is shown that this brilliant and tangible method follows and comprehends, in all their phases, transformations hitherto concealed under analytical forms mysterious as the oracles of an invisible power.

When the warriors of France, at the command of the man of the age, and guided by our scientific predecessors, ascended the river whose source is unknown, they suddenly discovered Thebes, that city of geometrical forms, broadly displayed by the rays of a tropical sun. The enthusiasm which struck them was similar to that which seized the young admirers of science, when they, at our periodical meetings, saw the learned modern unveil before them the monument of his geometry.

After these discoveries of the masters of the science, we may still mention, with honour, the numerous and beautiful researches in analysis and mechanics which we owe to MM. Coriolis, Duhamel, Liouville, and Sturm, young geometricians, on whom rest the hopes of the academy.

A profound analyst, M. de Corancez, who fell a victim to the fatal epidemic of 1832, left, as the last product of his studies, a learned Theory on the Motion of Water in Vessels. This has been published, since his death, under the auspices of our colleague, M. de Prony, who, during the period under consideration, has himself persevered in his varied, learned, and fruitful inquiries.

We shall first mention his memoir on the formulæ for determining the effect of a steam-engine worked expansively; in which this geometrician shows that he was the first, with Bettancourt, to assign a mathematical law to the expansive force of steam, under various degrees of temperature. We shall next allude to his application of the theory of particular solutions of differential equations to several questions of deep interest to engineers;—to his considerations on the effects which follow the contraction of the joints in large circular arches of bridges;—to his observations on the dilatability of long metallic chains; and—to his means of improving the proportional compasses, so as to extend the use of this instrument and give it more precision. We shall mention, as works of another class, the application of the calculus to the measurement of eddies and back-currents, produced by the narrowing of beds of rivers, on the introduction of bridge-piers, or by the construction of dams with a bottom-discharge;—his very remarkable examination of a system of embanking proposed at the mouth of the Seine, for the completion of the projects which had been entertained for years, and whose object was to render that river navigable by vessels of heavy burden; and, finally, his excellent memoir now printing by the *Administration Générale des Ponts et Chaussées*, in which M. de Prony describes a new system of embankment for rivers and large currents of water, of which he is the inventor.

These numerous works, whose titles alone exhibit their importance, shed lustre on a career of fifty years, devoted to great public works, and to the application of mathematics to the service of the arts. In the present year they have obtained a reward becoming to the dignity of a nation which places the glory of the sciences in the first rank of public duties. The enlightened justice of a monarch, whom mathematics applied to geography considers as one of its professors, regarding M. de Prony as the representative of the *Académie des Sciences*, has selected him for elevation to the peerage.

A worthy pupil of Monge and of Fourier has trod in their steps, and continued their train of discovery by his researches on transversals;—on the properties of projected figures; and, finally, on numerical calculation and the limits of series, directed to a practical object; on the last of which repose his highest claims to distinction. We shall merely refer to those water-wheels of a peculiar scientific form, which have become popular under the technical name of *Roues-à-la-Poncelet*. When their adoption shall be universal, and it is annually extending, France will have gained, by a simple theoretical improvement, the daily product of a motive power which cannot be estimated at less than 20,000 horses, or 120,000 men.

The labours of the officers of the artillery, and of the civil, military, and naval engineers, present admirable collections of calculations, of observations, and of undertakings. Limiting ourselves to the more remarkable, we shall mention, in civil engineering, the road weighing-machines, furnished with an improved dynamometer capable of comparison, and which the academy rewards in the present sitting;—a bridge, superior to any either in Europe or America, suspended from mountain *to mountain*, at Fribourg, in the midst of the Alps. In military *engineering* and in the artillery,—the theories of the pressure of earth and of arches, rendered more exact and applicable;—the laws of the issue of fluids from large orifices, studied and laid down from experiment;—the laws of the impulse of projectiles, and their penetration into solids of various natures, similarly sought after and calculated;—the friction of machines, submitted again to experiment, and the views of the illustrious Coulomb pursued to their whole extent; finally, in naval engineering,—a port, an arsenal, and vessels of the largest class suddenly created in Lower Egypt; and in the Thebaid,—the resources of naval art applied to the removal of the obelisk from Luxor as if it were but the unshipping of a mast; we may estimate the skill and simplicity of the means by the fact that eight persons were sufficient to regulate and check the lowering of an obelisk which weighed three thousand six hundred men.

Such are the works of Raucourt, of Changey, of Poncelet, of Lebros, of Piobert, of Morin, of Cérizy, and of Lebas, all students of the *Ecole Polytechnique!*

The Observatory of Paris is occupied by another swarm from the same immortal hive. It is there that the exquisitely beautiful laws of the intermission and the polarization of light were discovered, and their novel and striking applications to the nature of solar and of sidereal light were first made. It is there, particularly, that we must go to study the laws of the distribution of heat, as they follow the respective depths of the earth's strata. It is there that our astronomers have amassed, within the last few years, nearly a hundred thousand observations on the magnetic needle, for the purpose of determining the laws of its diurnal variation, both in the dip and in the declination; finally, it is there that the fact, so remarkable, of the disturbances of the magnetic motions by the aurora borealis, even at enormous distances, was first observed. Auroras, perfectly invisible at Paris, are now detected and noted at the observatory: subsequently, we learn that they have been witnessed at the extremities of Europe, and even beyond the ocean, on the continent of America.

The celebrated astronomer to whom we owe this discovery, has achieved for science another, and probably a far more difficult, conquest. The Observatory, though rich in instruments, which do honour to our recent skill, had not sufficient accommodation for carrying on the observations with certainty, facility, or security; in a word, it did not possess the conditions imperiously demanded by a science whose progress requires mathematical precision. He presented to the Chamber, of which he is a member, a representation of the discoveries to which his science aspired. He said that foreign countries were proceeding to their accomplishment with superior advantages, by the possession, not of genius, but, of scientific means. He simply inquired of the representatives of his country, whether they would, or would not, disinherit France of a glory which the human mind was ready to seize upon. At this noble language of the philosopher, pleading for science, the malignant spirit of party disappeared, the country and its fame were alone considered, and the generosity of the legislature instantly exceeded the request and the hopes of the astronomer. The government, excited by a similar emulation, commenced the work, without delay, and two years and eight thousand *pounds have been* sufficient to accomplish the whole with the *grandeur proper to France.*

The study of theoretical astronomy, so successfully prosecuted by M. Pontécoulant, and the recent correction of the masses of Jupiter and Saturn, have enabled the French geometrician to calculate the period of Halley's comet with far more precision than any learned foreigner; by a happy application of a method of Lagrange, he has rendered the calculations of the elements of this body, even when the influence of all the disturbing forces is taken into the account, brief and easy; a calculation hitherto terrific from its length and complexity. At the same time, the astronomers of Paris have established, by ingenious experiments, suggested by M. Arago, that the light of this comet is a reflected light; and they were the first to point out those singular and luminous sectors, which intermittingly appear and disappear on the edge of this body.

Let us now proceed to other works, intended, like the preceding, to confer reputation and benefit on the country; I am desirous to speak of those noble undertakings which are destined to describe, mathematically, the coasts, the territory, and the soil of France.

After having undertaken and completed, under the empire, the hydrography of the coasts of Belgium, and of Holland, and next that of the shores of the Ocean, from Ushant to Spain, M. Beautemps-Beaupré, our colleague, is now continuing, on the same plan, the hydrography of the shores of the Channel, and which will be followed by that of the Mediterranean. In three seasons he will have terminated the whole of the operations appertaining to the Atlantic survey, in which the rigour of the methods improved and employed by him, and the scrupulous precision of the observations on land, at sea, and, I may almost say, beneath the sea, and the double check of the calculations by different engineers, and, lastly, the beauty of the engraving, conspire to produce a work which will be worthy of the actual state of science and art. A word will be sufficient to enable us to appreciate the magnitude of the undertaking. Two hundred thousand pounds, thirty years of labour of the hydrographic corps, one half of the life of its chief, and four hundred and fifty quarto volumes of observations and calculations, have been necessary to accomplish the hydrographic surveys of the coasts of France, to complete them in their twofold relation to commerce and the naval interests, and to adapt them to the preparation of the grand atlas of the *Pilote Français*.

Less advanced than this, but demanding a yet greater amount of labour, is the new map of France. This is now under execution upon a general plan, which the illustrious author of the *Mécanique Céleste* stamped with a geometric character. M. de Laplace, selected for his genius by Napoleon, and welcomed by Louis XVIII., for his wit, exerted his high influence that the operations might be carried on by a corps of skilful geographers, educated in the *Ecole Polytechnique*. France was then scored, from frontier to frontier, with two new meridian chains of triangulations, and with six parallel ones, all observed with the most improved instruments, and calculated by methods that the recent advances of geodæsy and astronomy had furnished. These immense works were completed between 1818 and 1830.

The hydrographers, and M. Daussy in particular, have already measured the great sinuous chain which follows the shores of the Ocean.

In 1830, 1831, and 1832, the geographers measured that which runs along the Mediterranean, from Perpignan to Marseilles.

Memoirs, worthy of the undertaking, and which have been submitted o the judgment of the Academy, have described the results of the principal chains; that of Colonel Coraboeuf on the geodæsical operations in the Pyrenees, and the comparative level of the seas, has demonstrated, for the first time, the equal height of the Atlantic and the Mediterranean. The whole of the operations are given in the *Nouvelle Description Géométrique de la France*, the production of our colleague, M. Puissant, who, for many years, has had the scientific direction of the map in question.

Results of high importance have followed some of the measures consequent on these operations. The latter have been prolonged on the central meridian as far as the Orkneys on the north, and to the Balearic Isles on the south by MM. Biot and Arago; on the mean parallel they have been extended from the Atlantic Ocean to the Adriatic Sea. The results were described and demonstrated in a memoir, read by M. Puissant to the Academy in 1833, intituled *Nouvelles Comparaisons des Mesures Géodésiques et Astronomiques de la France*. They show, 1st, that the flattening of the earth, in that part which is occupied by the territory of France, is greater than had been previously estimated; 2nd, that the surface of this territory, considered as a whole, is not formed by two portions of the same spheroid, symmetrical on the east, and on the west, of the meridian of Paris. This meridian itself, and all the others which pass along France, are not plane curves, they, with the parallels, are found to have, within the limits of the kingdom, a decided double curvature; their tangential planes are inclined, some towards the west, others towards the east, of an imaginary plane meridian. M. Puissant gives these inclinations for the principal points of the great chains of the map of France. At the same time, the chain measured by Delambre and Méchain, has been corrected in its intermediate part, and in its final result on the base at Perpignan.

France divided into twenty-one quadrilaterals, by these great meridional and parallel chains, is proposed to be subdivided by a net-work of primordial triangles, having their summits coincident with the elevated points of the surface, and with the more remarkable edifices. A triangu-ation of a secondary order is then to connect these guiding-points, with ll those that are essential to the local topographies. Lastly, the grand national assessment, (*Le Cadastre*,) commencing with this last reticula-ion, will fill it entirely by the actual survey and measurement of all the distinct properties which occupy the soil of France.

When these triangulations shall be completed, the positions of forty housand points will be mathematically determined, and there will be given their azimuth, latitude, longitude, and altitude (that is, their height above the mean level of the sea).

These triangulations, it must be confessed, have proceeded but slowly since 1830. An act, of no very scientific character, dispersed the corps of *Ingénieur-géographes*, which had been exclusively filled up till 1831, from the *Ecole Polytechnique*. Henceforth confounded with the *officers of the staff*, and liable to services foreign to geometry, and more favourably regarded by power, the real geographers of the map of France will diminish in number, as their special occupation declines. They will no longer have that exclusive employment which exclusively leads to excellence. Let us, at least, give utterance to our hopes, that those who still survive, may be constantly employed to finish the great work which they commenced. It would be disgraceful to the nineteenth century, and to a government friendly to the arts, if, under its auspices, a distinguished undertaking of the fallen dynasty should degenerate as it advances towards completion, and if mere military expedition should be substituted, in works of this nature, for scientific labour. Let us declare openly, that neither the total cost, nor the graphic excellence, nor the magnificent scale of the plates, will alone constitute the map of France a monument of which the nation may be proud. The utmost rigour of mathematical precision must pervade the whole, and enter into the minutest detail of the operations. This is the kind of perfection for which the Academy must plead, as they would for a national distinction which ought to be sacredly maintained.

The mathematical position of all the important points being ascertained, there would remain to be described the nature itself of the soil. This is the object to be attained by the geological map of France, undertaken for the service of the natural sciences, and the productive arts. Attempted sixty years ago, under the patronage of Lavoisier, but suspended by the Revolution, it was recommenced in 1835, by three students of the *Ecole Polytechnique;* the first in rank as in age, M. Brochant, is already a member of the Academy and director of the undertaking, of which he furnished the plan; the second has been elected in the present year, in the present month even, to take his seat amongst us; the third stands in the first rank of those who were the competitors and rivals of his distinguished fellow-labourer.

MM. Elie de Beaumont and Dufresnoy have devoted ten years of travel, exploration, study, and discovery, in the examination of the whole territory of France, and of those of the adjacent countries of England, Spain, Italy, Switzerland, and Western Germany, for the purpose of collecting the materials of this map. It is so far advanced, that the primordial lines, the intersection of the principal strata, and the visible surface, are already drawn and engraved. When it shall have received the hatchings, the marginal boundaries, and the references, it will be in every respect equal, and in some respects superior, to the celebrated geological map of England.

The maps of the detail, which are intended to exhibit a complete and minute topography of our mineral riches, and which I shall designate the national geological assessment (*Le Cadastre Géologique*) of the country, have been commenced by the departments. These maps take for their marginal boundaries, and for their guiding-points, the principal lines and the fundamental data of the general map. Several of the departemental councils have already granted the necessary funds for these provincial undertakings: and there is far too much knowledge and love of excellence diffused among these elective bodies to permit the supposition, that any one of them will refuse to make the sacrifice from which will follow so much useful information, beneficial to agriculture, to com-rce, to manufactures, to the requirements of private life, and to the blic service.

Engineers, reared in the illustrious school of Monge, could not perpetually occupied in the study of immense and varied territories, ich it was their duty to describe, without indulging occasionally in application of the principles of the higher geometry, and of mechanics, geology. M. Elie de Beaumont has done this with the greatest suc-ss, in his novel and prolific notion of the successive elevation of the eat mineral beds which constitute the crust of the globe.

The application of the mathematical sciences to those which are signated the natural ones,—to the wants of the productive arts,—to blic works, of which we have presented such fine examples from works er which the Academy presides, forms the most remarkable character-ic in the actual progress of human knowledge.

The theory of heat promulgated by Fourier still excites attention, d receives further developements. It has been made the subject of a rge work by M. Poisson.

Effects, which the reduction of heat by chemical means cannot pro-ice, are now accomplished by mechanical agency. In 1830, the Academy warded the gas-compressing machine of M. Thilorier. Chemistry, by e use of this apparatus, may be furnished with carbonic acid gas, first a liquid form, and afterwards solidified. These transformations are tremely important.

A corresponding-member of the Academy, M. Melloni, has commu-icated to us new facts relating to radiant heat, and has reduced them to lculation. M. Biot, to whom we are indebted for a report, or rather for a arned treatise, on the experiments of M. Melloni, has, by his own labours, tended the application of mathematical analysis to chemistry. The henomena of circular polarization reduced to calculation, have been made se of by him to explain some remarkable facts in organic chemisty. This bject, so perfectly new to the world, has engaged, for many years, the ttention of M. Biot.

Franklin, Galvani, Volta, Œrsted, and Seebeck, were the observers f those primordial facts in electricity, galvanism, and magnetism, from hich have flowed such vast series of phenomena; but philosophers of rance were the first to discover the laws of their theory. On these are ased the reputation of Coulomb and M. Poisson, in electric statics, and hat of MM. Ampère*, Arago, Biot, Savart, and Savary, in dynamic lectricity.

From the midst of these philosophers, M. Becquerel has opened a ath for himself. He has attacked chemistry with weapons of his own pre-aration, to subdue her to the dominion of mathematical laws. Electricity nd galvanism are the powers, or to speak more correctly, the single ower, which he employs. Gifted by nature with extreme delicacy of he organs of sense, and exquisite power of observation, we may justly ntitle him the Wollaston of France. Apparatus, almost microscopic, is ufficient to enable him to measure the grandest operations of the powers which are the objects of his contemplation. He detects, and appreciates with astonishing sagacity, the two electricities, as they develop themselves in the production of chemical action. He has improved the thermo-electric pile, and given it additional value, by the importance of his applications. With metallic threads of extreme tenuity, he measures the temperature of the interior of animals and of man, compares their variations in different parts of the organic system, and even those of the same part when affected by disease. A process entirely novel, and extremely valuable to medical science.

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* Since dead.

During a visit to the frontiers of France and Italy, made by this philosopher and M. Breschet, the colleague of his later experiments, they seized the opportunity of applying their instruments and processes to other phenomena, the observation of which has enabled them to extend still further, the domain of science.

It is by these means that the applications of mathematical science are extended, even to the phenomena of animal organization.

Acoustics are now employed to examine, the intensity, the variety, and the rhythm, of sounds, emanating from the heart, and from the lungs, for the purpose of deciding upon the healthy or the diseased state of those organs; the nature and degree of any morbid affection, are also ascertained by the same agency.

M. Magendie, our colleague, has been induced, by some ingenious reasonings, to borrow from mechanical laws his explanation of the normal sounds of the heart, and which he refers to the oscillation of that organ.

Our colleague, M. Flourens, seeks, in the mechanical pressure exercised upon the brain, an explanation of the condition of those persons who undergo the operation of trepanning, and even of the effects of the operation. With the skill of an able *physicien*, he studies the respiration of fishes, and explains the hydrostatic process by which these animals inhale more free oxygen in water, which appears to afford them so little, than they do in the atmosphere, where this vital gas is in abundance.

M. Dutrochet, who has communicated to us so many facts, the fruit of his ingenious observations on the internal dynamics of vegetables, has pushed his investigations into the mechanism of the respiration of insects, both aquatic and aërial. With this view he has examined the chemical processes which are carried on during this act of their existence.

I am far from having enumerated all the recent modes in which mathematics have been applied to natural science. I have not even hinted at its application to that of politics, and of social economy, nor to the subject of population. Having individually taken a part in these discussions, I shall pass them unnoticed.

But, in this sketch, rapid, incomplete, imperfect, I ask with confidence, Do you not recognise the ever-increasing utility of science, the extent of her benefits, the sublimity of her titles, even during the short, embarrassed, and turbulent period to which I have confined myself? The sciences must have, therefore, of necessity, a vital energy peculiar to themselves; a progressive power, superior to the obstacles of time, of things, and of man. Human passions, vulgar ambition, and party-interests, pass away, but the labours of science, the sacrifices made for her sake, the victories borne off in her name, remain; and contribute to *the enlargement* of that splendid and profitable heritage on which, at the *present day*, is based her real grandeur.

A POPULAR COURSE OF GEOLOGY.

III.

The Discoveries of Geology not opposed to Sacred History.

We have now enumerated some of the advantages of geology, and some of the pleasures attending the study of it; and is it to be wondered that a science possessing such attractions, so important in its results, so fertile in new discoveries, and whose lofty and boundless speculations possess an interest almost poetic, should be remarkable for the enthusiasm with which it inspires its votaries, or that their number should daily increase? There are those, however, who, admitting all that can be urged in its favour, would yet dissuade us from the study, as opposed to the highest and best interest of mankind, by tending to undermine our belief in the truth of the Sacred Records. To these objectors, whose motives we must respect though we cannot applaud their judgment, we would reply, that their advice appears to argue a lurking distrust of the truth of what they profess to believe; for it amounts to a recommendation that we should close a great and important chapter in the volume of God's works, lest it should be found at variance with the volume of his Word. Conflicting falsehoods there may be; but truth can never be opposed to truth, however they may differ in their principles, and in the means by which we obtain a knowledge of them. We could never have become acquainted with the relations of man to his Creator, and with God's Providence in his dealings with man, which is moral truth, except by means of Revelation; neither in the absence of a revelation of physical truth, can we discover those secondary causes, by means of which the material world is governed, except by observation, experiment, and induction; and as long as we adhere honestly to that cause we never can arrive at anything but truth. The study of the material world is nowhere prohibited, but, on the contrary, has the express sanction of many passages of Scripture. *Great*, says the Psalmist, *are the works of the Lord, sought out of all them that have pleasure therein;* and he never breathes more rapturous devotion, nor rises to loftier strains of poetry, than when expatiating on the beauty and the wonders of creation. It is true there are some natures which convert into poison that which to others is wholesome nutriment, and there are some minds which betray a morbid eagerness to discover in every new fact in physical science an argument against the truth of Revelation. But are we to abstain from what is in itself good because others pervert it to evil? If we, from a laudable, though mistaken zeal, abjure the study of natural science, the infidel and the sceptic will not. They, on the contrary, will pursue it the more eagerly the more likely they think it to afford a position from which revealed religion may be successfully assaulted. Surely, then, the believer, instead of leaving them in possession of the field, ought to consider the study of natural science an imperative duty, that he may be able to foil such assailants with their own weapons; for if he enter the contest, as too many injudicious advocates have entered it, unacquainted with the true nature *of the phenomena* bearing on the question, and ignorant of *the conditions of the problem* he undertakes to solve, he can only bring discredit on the cause which he attempts to serve. The assailants of Revelation usually assume, and too many of its defenders argue on the assumption, that we have reason to expect a system of physical science in the Sacred Writings; but the slightest consideration of the purpose for which they were given, must convince us that such a revelation would have been quite at variance with their professed object. That object was to make man acquainted with his relations to his Creator—with his original state—his present condition—his future hopes. Such a communication contained too much that was humiliating to human pride, to obtain a ready acceptance, notwithstanding the high credentials by which it was supported; and to have connected with it a system of physical science, opposed to the prevailing notions respecting the material world, instead of facilitating its reception, would have afforded fresh grounds for incredulity, by the announcement of facts apparently contradicted by the evidence of the senses. Moreover, to have made physical truth the subject of revelation, would have destroyed its great use as an exercise of the reasoning powers; and would have deprived it of its highest interest, by superseding all discovery, and leaving to us only the task of committing to memory what was plainly recorded.

The object of Natural Religion is to trace the Author of Nature in his works; and with this physical science has an intimate connexion. The object of Revealed Religion is the providential history of man; and with this natural science has only an incidental connexion, arising out of such physical facts as are mentioned in the course of that history.

We have already briefly noticed the manner in which geology confirms and extends the evidences of natural religion. Before we proceed to examine its bearings on the few physical facts recorded in the Inspired Writings, let us consider the case of astronomy with respect to those writings. Astronomy may be regarded as a nearly perfect scienc; and its conclusions rest upon a basis which cannot be shaken—mathematical demonstration, the highest degree of proof, of which truth is susceptible. Yet we do not find in the Scriptures the remotest hint by which even a clue might be obtained to those great astronomical truths, which have been slowly and laboriously worked out by the process of observation and induction. On the contrary, every notice of those subjects is such as to favour popular notions, derived from the first sensible appearances of the heavens and the earth. But surely we are not therefore called upon by true religion to reject a system of physical truth, founded on the clearest demonstration, any more than we are required by sound philosophy to disbelieve a revelation of moral truth, supported by its appropriate train of moral evidence and credible testimony. Ought we not rather to consider the manner in which the phenomena of the natural world are spoken of in the Scriptures as a proof, that the moral destiny of man is all that has been revealed; that the phenomena and laws of the material world are left to be discovered by human reason; and that, when speaking incidentally on subjects connected with physical science, the language of Inspiration has been accommodated to popular notions and sensible appearances, in the same manner that when speaking of his own incomprehensible attributes the Almighty has adopted language suited to human comprehension.

No sane person would now attempt to overthrow the modern system of astronomy by appeals to the text of Scripture. But astronomy was not always in its present matured state. It has been said of our science that it is scarcely yet out of its cradle; and its present state may be compared to that of astronomy, when the discoveries of Galileo were deemed hostile to religion. But, after all, the facts hitherto brought to light by the researches of geology, are more in accordance with the text of Scripture than are those of astronomy. What can be gathered from the brief account of the creation, contained in the first chapter of Genesis, more than this; that the world was not self-existent and eternal; that it was called into being by the fiat of an Almighty Creator; and that, though he could have produced it in an instant, clothed, as we now behold it, with plants, and furnished with inhabitants, it was his pleasure to proceed gradually in the work of creation; and that man was the last, as he is the noblest, of his Maker's works? And what, we would again ask, is there in the phænomena of geology inconsistent with this? It may be doubtful whether we see the remains of the earliest races of beings by which the waters were inhabited; and there may be a difference of opinion, in the present state of our knowledge, whether there were any animals of the higher orders contemporary, as inhabitants of the land, with those marine races, whose remains we find in the ancient state; but the recent origin of man, as we have before shown, does not admit of a doubt. On that point all geologists are agreed,—those who believe the Sacred Records and those who do not; and, as Cuvier has said, we cannot, from geological monuments, date the introduction of the human race upon the earth at a more remote period than about six thousand years ago.

With regard to the time occupied in the gradual work of creation, there have been differences of opinion arising out of the text itself, independent of any geological investigations; and though the majority of commentators have maintained, that by the days of creation are to be understood periods of time equal to that in which the earth performs a revolution on its axis, others have contended that they might be periods of long and definite, though, to us, unknown duration.

But this extension of the term is by no means necessary; for the phenomena of geology may be reconciled with the Mosaic account of the creation by the supposition that, after God had, "in the beginning," created the heaven and the earth, a long, undefined period, may have elapsed, during which a series of revolutions may have taken place, which are passed over in silence, as being totally unconnected with the history of the human race; that, at the termination of this period, the earth was in the chaotic state described in the second verse of Genesis; and that, at this point, the narrative of the six days of creation commences; during which the earth was again restored to order, covered with the existing races of plants and animals, and finally made the abode of man. On either interpretation of the term day, whether it be restricted to days of twenty-four hours, or be extended to periods of definite but unknown duration, a difficulty will remain, connected rather with astronomy than with geology. This difficulty is, that according to the received interpretation, *the sun was not* created till the fourth day. If, therefore, the word day is to be taken in its ordinary acceptation, those days that succeeded the creation of the sun must have been very different from those that preceded that event; and if, by days, we are to understand long periods, then, during three of those long periods, the earth must have existed independent of those laws of gravitation of which the sun is, to our system, the centre, and not only must it have so existed, but during one of those long periods it must have been clothed with vegetation, "herb yielding seed, and fruit-tree bearing fruit," without the aid of solar influence. But this difficulty will be removed by adopting the interpretation suggested by Dr. Buckland, in his recently-published *Bridgewater Treatise;* that the sun and moon were not called into existence on the fourth day; that they were then prepared and ordained to certain offices of high importance to mankind, who were shortly to be created. According to this interpretation, the heaven and the earth, declared in the first verse to have been created by God in the beginning, mean the universe, including the sidereal systems, with the planets and their satellites; the earth being specially mentioned, as the subsequent scene of those creative operations which were about to be described. A long, undefined period, to which we have before alluded, between this primitive creation and the present order of things is supposed to have terminated; and with it to have terminated a long series of geological epochs. This would be the commencement, or evening, of the first day, when the earth lay in confusion and emptiness, the wreck and ruins of a former state, involved in temporary darkness and a temporary state of submergence. The work of the first morning was to dispel the darkness that overspread the ancient earth. On the third day the waters were commanded to be *gathered into one place* and the dry land to *appear;* but neither land nor waters are said to have been created on that day. The same interpretation may be applied to the fourteenth and four following verses; what is there stated respecting the celestial luminaries appearing to be spoken without any regard to their real importance in the universe, but solely with reference to our planet and the human race. Light may have been re-admitted to the earth on the first day, the exciting cause of light remaining obscured by mists which hung over the earth, and the further purification of the atmosphere, on the fourth day, may have caused the sun, moon, and stars to re-appear in the firmament, and to assume their new relations to the human race and the re-constructed earth.

The declaration, in this place, that God made them, will then merely be a repetition and amplification of part of the first verse, intended to warn the Israelites against that early and prevalent superstition which led to the worship of the host of heaven. These explanations, by which the letter of the text of Genesis is reconciled with geological phenomena, are supported by Dr. Buckland, by the authority of Hebrew critics, and of several learned and pious divines. He further shows that it had long been a question among theologians, whether the first verse of Genesis ought to be considered, prospectively, as a summary announcement of that creation, the details of which are given in the records of the six successive days, or as an abstract statement, that the universe was created by God, without defining the period when that creation took place; and is also stated, on the authority of the Regius Professor of Hebrew, at tford, that, in some old editions of the English Bible, in which there no division into verses, there is a break at the end of what is now the cond verse; and that, in Luther's Bible (Wittenberg, 1557), there is, oreover, the figure 1 placed against the third verse, as being the ginning of the account of the creation of the first day.

Referring those who are desirous of seeing this question more fully scussed, to the interesting volume from which the above abstract is ken, we shall conclude this part of the subject in the words with which r. Buckland concludes his chapter on the consistency of geological scoveries with Sacred history.

"After all," he says, "it should be recollected that the question is t respecting the correctness of the Mosaic narrative, but of our inter- etation of it; and still further it should be borne in mind, that the ject of this account was not to state *in what manner*, but by *whom*, the orld was made. As the prevailing tendency of men, in those early ys, was to worship the most glorious objects of nature, namely the n, and moon, and stars, it should seem to have been one important int in the Mosaic account to guard the Israelites against the polytheism d idolatry of the nations around them, by announcing, that all these agnificent celestial bodies were no gods, but the work of one Almighty reator, to whom, alone, the worship of mankind was due*."

Having thus endeavoured to show that, as regards the recent origin the human race, the researches of geologists confirm the Sacred narra- ve; and that the high antiquity which they claim for the globe, on the idence of a vast series of extinct races of organized bodies, entombed the earth's crust, is by no means inconsistent with the literal interpre- tion of that narrative, the only remaining event mentioned in Scripture hich can have any connexion with geology is the general Deluge.

Phenomena of two opposite kinds have, at different times, been ap- ealed to as evidences of that event,—the organic remains contained in e stratified rocks, and the loose covering of gravel, clay, and sand, dis- ibuted over the surface of the earth.

When the study of the structure of the earth first began to attract e attention of Europe, in the beginning of the sixteenth century, the arine remains discovered gave rise to much discussion; some main- ining that they were not real remains, but mere sports of Nature, rmed by what they called a "plastic force," which had power, it was id, to fashion stones into the forms of organic bodies. Others con- nded that they were the real exuviæ of animals that had lived and ied on the spots where their remains occurred; and others again con- nded that they were left in their present situations by the Noachian eluge, to the transient action of which they attributed the formation of e stratified rocks, in all their vast thickness, with their almost infinite ibdivisions, and with all their successive groups of extinct races.

More than a century was spent in proving the animal origin of ese remains, and in refuting the notion of their having been produced y a "fermentive process" or "a plastic virtue," and for more than nother century and a half, the discussion was maintained whether they were the remains of animals gradually accumulated at the bottom of the sea, subsequently converted into dry land, or whether they were left by the deluge of the sacred writings. The latter opinion was not finally abandoned by geologists till the close of the eighteenth century; when Werner, in Germany, and Smith, in England, had, independent of each other, established the order of succession of the stratified rocks; and Smith had proved that each group of strata is characterized by its peculiar group of organic remains. The researches of Cuvier among the extinct animals of the tertiary formations of the Paris basin were likewise affording additional proofs of the gradual accumulation of strata, and of the remoteness of the forms of mammalia embedded in even these comparatively modern rocks, from any forms now living on the face of the earth.

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* *Buckland's Bridgewater Treatise*, vol. i., p. 33.

About the same time, the notice of observers was attracted to the water-moved gravel, and other phenomena indicating the passage of a large body of water, at a recent epoch, over a great part, not of Europe only, but of the northern hemisphere. This gravel is composed chiefly of fragments of the rocks of the neighbourhood in various states of attrition, mixed with others which must have been drifted from a distance, and are now found considerably to the south of the rocks from which they have been derived.

Huge blocks of granite and other rocks dispersed through and lying upon this gravel, are found in situations far to the south of their parent-rocks; and have frequently been lodged, in a most extraordinary manner, high on the flanks of chains of mountains, between which and the chains from which they were derived, wide and deep valleys intervene. Bones of mammiferous quadrupeds, of existing genera, but of species extinct, or existing at present only in warm climates, are found in this diluvium, mixed with some that are now inhabitants of Europe; and, within the last five years, fragments of marine shells, of existing species, have been discovered in it, in various parts of Great Britain and Ireland, sometimes at considerable heights.

These evidences of diluvial action were next connected by geologists with the Noachian Deluge, and with much more probability than the origin of the stratified rocks had been referred to that event. This hypothesis, first propounded by De Luc and other observers, was warmly taken up by Dr. Buckland, who gave to this transported gravel the name of diluvium. The new diluvial theory obtained the support and sanction of Cuvier, and soon became popular among geologists. There were some few, however, who ventured to dissent from it even during the height of its popularity, objecting to it that it represented the flood of the Scriptures as an impetuous torrent which had greatly modified the surface of the earth, excavating valleys, transporting immense blocks of rock, and covering hills, as well as valleys, with vast accumulations of debris; whereas it should appear from the brief narrative of that event in Genesis, that the rise and retirement of the waters was gradual, and even that vegetation was not destroyed by them. It was objected, moreover, that in the gravel supposed to have resulted from this Deluge, no *human remains* or works of art had ever been found, but only the *remains of animals*, a large proportion of which belonged to extinct species.

As our knowledge of the tertiary strata extended, it was found that lacustrine and marine deposits, of gradual accumulation, and belonging to different epochs, had been confounded with this diluvium. There appeared, too, reason for supposing that some of the extinct volcanic cones of central France, and some of the minor cones on the flanks of Etna, were formed at an era more remote than that of the historic Deluge, or the human race; and they exhibited no traces of diluvial action, though consisting of loose scoriæ and sand, which could not, for a moment, have resisted the rush of a violent flood. The investigations, moreover, of Elie de Beaumont showed that the mountain-chains of Europe had been elevated at several distinct epochs; and geologists began to connect the superficial gravel with these periods of elevation, supposing that, in some cases, it had been shot off the flanks of the nearest mountains during their elevation, or that the violent agitation into which parts of the ocean had been thrown by the sudden upheaving of its bed, had given rise to great waves which had rushed over the land.

The hypothesis which attributed all the superficial gravel in the world to one event, and that the Noachian deluge, was abandoned successively by those who, for a time, had been some of its most zealous supporters. Professor Sedgwick renounced it from the chair of the Geological Society, in the year 1831, in the following words:—

"Bearing upon this difficult question, there is, I think, one great negative conclusion now incontestably established,—that the vast masses of diluvial gravel scattered almost over the surface of the earth, do not belong to one violent and transitory period. It was indeed a most unwarranted conclusion when we assumed the contemporaneity of all the superficial gravel upon the earth. We saw the clearest traces of diluvial action, and we had, in our sacred histories, the record of a general Deluge. On this double testimony, we gave unity to a vast succession of phenomena, not one of which we perfectly comprehended, and under the name of diluvium, we classed them all together.

"To seek the light of physical truth by reasoning of this kind is, in the language of Bacon, to seek the living among the dead, and will ever end in erroneous induction. Our errors were, however, natural, and of the same kind which led many excellent observers of a former century to refer all the secondary formations of geology to the Noachian Deluge. Having been a believer, and to the best of my power, a propagator of what I now regard as a philosophic heresy, and having more than once been quoted for opinions I do not now maintain, I think it right, as one of my last acts before I quit this chair, thus publicly to read my recantation.

"We ought, indeed, to have paused, before we first adopted the diluvial theory, and referred all our old superficial gravel to the action of the Mosaic Flood. For of man, and the works of his hands, we have not yet found a single trace among the remnants of a former world entombed in these ancient deposits. In classing together distant unknown formations under one name; in giving them a simultaneous origin, and in determining their date, not by the organic remains we had discovered, but by those we *expected hypothetically* hereafter to discover in them; *we have given one more* example of the passion with which the mind fastens upon general conclusions, and of the readiness with which leaves the consideration of unconnected truths.

"Are, then, the facts of our science opposed to the Sacred Record? And do we deny the reality of an historic deluge? I utterly reject such an inference. Moral and physical truth may partake of a common essence; but, as far as we are concerned, their foundations are independent, and have not one common element. And in the narrations of a great fatal catastrophe, handed down to us, not in our sacred books only, but in the traditions of all nations, there is not a word to justify us in looking to any mere physical monuments as the intelligible records of that event; such monuments, at least, have not yet been found, and it is not, perhaps, intended that they ever should be found. If, however, we should hereafter discover the skeletons of ancient tribes, and the works of ancient art buried in the superficial detritus of any large region of the earth; then, and not till then, we may speculate about their stature, and their manners, and their numbers, as we now speculate among the disinterred ruins of an ancient city.

"We might, I think, rest content with such a general answer as this. But we may advance one step further,—History is a continued record of passions and events unconnected with the enduring laws of mere material agents. The progress of physical induction, on the contrary, leads us on to discoveries, of which the mere light of history would not indicate a single trace. But the facts recorded in history may sometimes, without confounding the nature of moral and physical truth, b brought into a general accordance with the known phenomena of nature and such general accordance I affirm there is between our historical traditions and the phenomena of geology. Both tell us, in a language easil understood, though written in far different characters, that man is recent sojourner on the surface of the earth. Again, though we hav not yet found the certain traces of any great diluvial catastrophe, whic we can affirm to be within the human period; we have, at least, show that paroxysms of internal energy, accompanied by the elevation of mou tain-chains, and followed by mighty waves desolating whole regions the earth, were a part of the mechanism of nature. And what h happened, again and again, from the most ancient, up to the most mode periods in the natural history of the earth, may have happened onc during the few thousand years that man has been living on its surfa We have, therefore, taken away all anterior incredibility from the fact a recent deluge; and we have prepared the mind, doubting about t truth of things of which it knows not either the origin or the end, the adoption of this fact on the weight of historic testimony*."

Dr. Buckland himself has, at length, abandoned the theory, whi through him, obtained so much celebrity. We will give his recantati likewise, in his own words, taken from a note on this subject in *Bridgewater Treatise*.

"The evidence which I collected in my *Reliquia Diluviana*, 18 shows that one of the last great physical events which have affected surface of the earth, was a violent inundation, which overwhelmed gr art of the Northern Hemisphere; and that this event was followed by he sudden disappearance of the species of terrestrial quadrupeds which had inhabited these regions in the period immediately preceding it. I also ventured to apply the name diluvian to the superficial beds of gravel, clay, and sand, which appear to have been produced by this great irruption of water.

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* Sedgwick's "Anniversary Address, 1831;"—*Proceedings of the Geolog* Society, vol. i., p. 313.

"The description of the facts that form the evidence presented in this volume, is kept distinct from the question of the identity of the event attested by them with any deluge recorded in history. Discoveries which have been made since the publication of this work show, that many of the animals therein described existed during more than one geological period, preceding the catastrophe by which they were extirpated. Hence it seems probable, that the event in question was the last of the many geological revolutions that have been produced by violent irruptions of water, rather than by the comparatively tranquil inundation described in the Inspired Narrative.

"It has been justly argued against the attempt to identify these two great historical and natural phenomena, that, as the rise of the waters of the Mosaic deluge is represented to have been gradual, and of short duration, they would have produced comparatively little change on the surface of the country overflowed. The large preponderance of extinct species among the animals we find in caves, and in superficial deposits of diluvium, and the non-discovery of human bones along with them, afford other strong reasons for referring these species to a period anterior to the creation of man. This important point, however, cannot be considered as completely settled, till more detailed investigations of the newest members of the Phocene, and of the diluvial and alluvial formations shall have taken place."

If, then, we be asked, are there no traces upon the earth of the Deluge of the inspired writings? we can only reply, in the words above quoted from Professor Sedgwick, that none have yet been found, and that perhaps it is not intended that they ever should be found; or we may adopt the argument respecting the Mosaic Deluge contained in the following extract from Mr. Lyell, who considers that there are not sufficient geological data for inferring that instantaneous upheavement of mountain-chains, and of great waves arising therefrom, which are considered by Professor Sedgwick, to have removed the anterior incredibility of an historic deluge.

"I may observe," says Mr. Lyell, "that the reasoning above alluded to seems to proceed entirely on the assumption that the Flood of Noah was brought about by *natural* causes; just as some writers have contended that a volcanic eruption was the instrument employed to destroy Sodom and Gomorrah. If we believe the Flood to have been a temporary suspension of the ordinary laws of the natural world, requiring a miraculous intervention of Divine Power, then, it is evident that the credibility of such an event cannot be enhanced by any series of inundations, however analogous, of which the geologist may imagine that he has discovered the proofs. For my own part, I have always considered the Flood, when its *universality, in the strictest* sense of the term, is insisted upon, as a *preternatural event, far beyond* the reach of philosophical inquiry, whether as to the causes employed to produce it, or the effects likely to result from it. At the same time, it is clear, that they who are desirous of pointing out the coincidence of geological phenomena with the occurrence of such a general catastrophe, must neglect no one of the circumstances enumerated in the Mosaic history, least of all, so remarkable a fact, as that the olive remained standing while the waters were abating*."

We trust enough has now been advanced to convince the most timid that there is nothing to be feared for the cause of revealed Religion from the investigations of geologists, though they ascribe to the earth a higher antiquity than has been supposed to belong to it, in consequence, as we conceive, of an erroneous interpretation of the first two verses of the Bible; or, though they declare that they have not seen, on the earth's surface, any traces of a general deluge. No uneasiness is now felt, because the language of Scripture, with respect to astronomical phenomena is not in accordance with the present state of our knowledge respecting the universe. The explanation offered, in that case, by commentators on the Bible, is that "the sacred writers were inspired to speak of natural things with philosophical exactness, but were left to use popular language, and to discourse of them according to their appearances†," and the only reason that any alarm is felt at the wonders brought to light by Geology is their novelty. When they shall have had the sanction of another century, or even less, they will not be deemed more dangerous to religion than the discoveries of Newton.

We shall not now enter into further discussion respecting this diluvial gravel, because it is a subject which will demand a separate chapter, before we close this series. The subject, moreover, is at present undergoing much investigation, and discoveries are daily brought to light which promise, eventually, to lead to a solution of this difficult question; which is a question of much interest, because this diluvium is the first deposit we meet with in the descending series, and because its origin and history involve another important and interesting question, namely, whether causes now in action, and acting with no greater intensity than at present, are adequate, if sufficient time be allowed, to have produced all those changes on the surface of the earth which have taken place in past ages.

We shall now merely state our dissent from the theory, which attributes the dispersion of this gravel to a succession of waves, caused by the rise of mountain-chains, and to that theory which supposes it to have accumulated, during a long-protracted epoch, at the bottom of the sea, subsequently laid dry, either by the slow elevation of continents, such as appears to be now going on in Scandinavia, or by more violent subterranean movements, acting with unequal intensity, which others have recourse to in order to account for the different heights at which this gravel occurs. We shall, at the same time, place on record our conviction, that some unknown cause, not now in action, produced a violent rush of the ocean, from the north, over a great part of the northern hemisphere, at a period when the leading geographical features of our present continents were established, when the waters were inhabited by the existing species of Molluscæ, and when a large portion of the mammiferous inhabitants of the land belonged to extinct species, or to species only inhabiting the warm regions of the earth, and which disappeared suddenly with this catastrophe. But, while we contend that by this violent inroad of the sea, fragments of marine shells, of existing species, were lodged on the surface of previously-existing land at the greatest height[*] at which they have hitherto been found in this tumultuous deposit, we by no means deny that this inundation may have been accompanied and succeeded by slight and local elevations of the land; so that in one place, on the coast, the diluvium shall be found resting on an ancient beach, now seventy feet above the level of the sea; and in another case, likewise on the coast, shall be covered at about the same height by a thin deposit, containing marine shells, which seem to have lived and died where they are found.

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* *Principles of Geology.* 4th Edition, vol. iv., p. 219.

† *Scott's Bible.* Note on Genesis i. 16.

The evidence on which these opinions are founded will be reserved for a subsequent part of this series; and there is good reason to hope, from the investigations now in progress, that in the mean time, the question will be decided; and that either our views above stated will be confirmed, or will be proved to be erroneous, in which latter case we shall endeavour to read our recantation with the same frankness which reflects so much honour on the illustrious geological chiefs whose declarations we have quoted.

We have now concluded the introductory part of our work. If we followed the usual course pursued at the commencement of an elementary treatise on geology, we should enter into a history of the rise and progress of the science, beginning with an account of the cosmological opinions of the Egyptian priests, of the Indian Brahmins, and of the philosophers of Greece and Rome; introducing, as a matter of course, that hacknied quotation from Ovid,—so hacknied, that we do not remember to have opened any introduction to Geology in which it did not occur,—

> . . . . Vidi factas ex æquore terras
> Et procul a pelago conchæ jacuere marinæ.

We should then proceed to show how, when the Roman empire was overthrown by barbarians, Geology—such as it was—took refuge in Arabia; and how, on the revival of learning in Europe, it first re-appeared in Italy, whence we should trace its progress into Britain, France, and Germany. We should expose the folly, though it passed for wisdom in its day, of the cosmological dreams of Burnet, Whiston, and Buffon. We should show how Werner advanced, and how he retarded, the progress of geological knowledge, and should enter into a history of the controversies which agitated the rival schools of Freyberg and Edinburgh; in which the names of Neptune and Vulcan, of Werner and Hutton, were the watchwords and the rallying cries,—controversies which, to the scandal of science, were conducted, not in the calm spirit of philosophical inquiry, but with all the warmth, and intemperance, and bitterness, of a borough election, or a vestry squabble. We should then detail the labours and noble generalizations of Smith and Cuvier, and advert to that grand epoch in the science, the formation of the Geological Society of London; when, wearied with the Neptunian controversy, geologists were convinced that the time was not yet come, when they could form a true theory of the earth; but that they must be contented, for a long time, to devote themselves to the observation of facts and the accumulation of data for future generalizations; and we should then point out the mighty results which have followed from a steady perseverance in this system, during thirty years, so that the progress of Geology has, by these means, been greater, during that period, than during the three centuries which preceded it. But to commence a work professing to teach the elements of Geology, or of any science, with a history of its rise and progress, appears to us inconvenient, to say the least of it, because it supposes, in those who are addressed, a certain previous acquaintance with the very subject they require to learn. Our object is to guide our readers into the plain and straight-forward road to geological truth, by setting before them the real discoveries of the science; and, surely, that object is not likely to be advanced by first leading them aside into every by-path of error in which they can possibly lose their way, recounting all the absurdities into which men fell, when, giving rein to the imagination, they constructed systems of cosmogony upon the foundation of a few ill-observed facts, and often without the observation of any facts at all. Our object is to exhibit the amount of light at present possessed by geologists, and it would be but a waste of time to show how, and how long, they groped about in darkness before they found that light.

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[*] *Moel Tryfan*, in Caernarvonshire, 1392 feet.

We agree with Professor Phillips that "the history of the progress of opinions in Geology may be useful as a warning to men advanced in geological inquiries not to reason upon assumptions while facts remain to be explored, and to repress that impatience of spirit, which ever seeks to anticipate observation by the efforts of invention; but that the student ought, if possible, to be kept in impartial ignorance of these conflicting hypotheses which are apt to fascinate the young and imaginative mind*." We shall, therefore, defer for the present, or wholly omit, a history of the rise and progress of Geology; and in our next number shall proceed to the consideration of the materials of which the earth's crust is composed, and of the order in which they are arranged.

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[*] Phillips's *Guide to Geology*, p. 5.

THE GREAT EARTHQUAKE IN CHILE, IN 1835.

By Alexander Caldcleugh, Esq., F.R.S., &c.

The phenomena attending this great disturbance of the surface of the earth have been so varied, and the extent of its effects so considerable, that I should almost deviate from my duty if I did not endeavour to draw up and transmit to the Royal Society some account of a convulsion which has laid in ruins three provinces, and caused incalculable damage to the southern part of this country. I am the more inclined to take this step, from a happy concurrence of circumstances having drawn several scientific observers to Concepcion shortly after the catastrophe, who have obligingly confided their notes to me. I trust, therefore, the Royal Society will not consider that I am about to trespass upon its time.

An idea, in some degree fanciful, prevailed for some time after the conquest of these countries by the Spaniards, that these convulsions of the earth's crust occurred at intervals of a century; afterwards it was supposed that about fifty years was the term which usually elapsed between great shocks; but, since the commencement of this century, the repeated catastrophes which have occurred, especially in the years 1812 in Caraccas, 1818 in Copiapo, 1822 in the province of Santiago, 1827 in Bogota, 1828 in Lima, 1829 in Santiago, and 1832 in Huasco, have prepared the minds of the inhabitants to expect at all times these frightful oscillations of the earth, which, although they cause little sensation at first, after some time affect the nerves in a manner not easy to account for by ordinary causes. That they happen at all times and in all states of the atmosphere, seems clearly decided. The finest weather, and the most variable, equally prevail at the moment; but many are the fancied signs hy which the coming earthquakes are predicted, and in the faith of which the inhabitants confide, as they think their experience bears them out. While some place great confidence in rats running violently over the ceilings of the room, others prepare for a shock when they observe the stars twinkling more than usual, and all fears are removed when much lightning coruscates in the Cordillera. As far as my own observations go, little reliance can be placed on the two former prognostics; something more certain seems to be due to the latter. A few hours previous to the earthquake which I am about to describe, immense flocks of sea-birds proceeded from the coast towards the Cordillera, a circumstance which occurred prior to the great shock of 1822; and it is affirmed by too many respectable persons not to be entitled to some degree of credit, that on the morning of the convulsion all the dogs disappeared from Talcahuano.

The summer in Chile had been rather colder than in preceding years. The mean of the thermometer in Santiago (two thousand feet above the level of the sea) for the months of January and February was 72° of Fahr. The mean of the barometer for the same period was 28·25, which is about one-tenth of an inch below its usual height.

From the 1st of February the barometer was unusually low in *Santiago; and on the 14th,* six days prior to the earthquake, the barometer, at half past 6 A.M., stood at 28·1, the thermometer at the same time being 73°. A slight oscillation, which lasted twenty seconds, was felt on this day; on the 20th the barometer marked 28·17, and the thermometer rose to 76°: the weather fine. In Concepcion, in the night of the 17th to 18th the barometer fell four-tenths of an inch, but gradually recovered itself, and indicated nothing extraordinary on the morning of the 20th. In Valdivia, according to the observations most obligingly communicated to me by Captain Fitzroy, of the Beagle Surveying Ship, the barometer stood on the 16th of February at 29·92, and continued to rise gradually until the end of the month, with an increased temperature. From my own observations, deduced from many oscillations, I have remarked that the barometer usually falls shortly before any considerable shock, and then returns to its ordinary mean. On the 26th of September, 1829, a very severe earthquake was experienced in this city, which did much damage to most buildings; the front of the house I then inhabited fell down; and it is worthy of remark, that the instant after every shock a burst of rain fell, which soon moderated, until a fresh tremor caused it to recommence.

The igneous vents of the whole range of the Cordillera may be said to have been in remarkable activity both preceding and at the moment of the late convulsion. From the flat-topped volcano of Yanteles, in front of Chiloe, to the lofty range of the Andes in Central America, all the information which has been obtained gives details of violent eruptions. On the 20th of January the volcano of Osorno, north-east of Chiloe, burst forth with inconceivable fury; and the lava was seen at night rushing out of the crater and rolling down the side of the mountain, elevated 3900 feet above the level of the sea. The reflection of the flame reached double that height, and is described to me by Mr. P. G. King, of the Beagle, as presenting the most magnificent object he had ever beheld. From the plains of Talca, eighty leagues to the south of the capital, two volcanoes were observed in activity a few days after the 20th of February. They are both situate near the lake of Mondaca, twenty-five leagues eastward in the Cordillera: and another new rent was observed on the estate called Cerro Colorado, on the right bank of the river Maule, and near its source. The volcano of Petoroa, and another near it, whence a stream of asphaltum flows, and those of Maypu and Aconchagua, have also been for some months in a state of activity.

In the month of January the volcano of Cosegüina in Central America became exceedingly active, and ejected a body of lava which covered a circumference of eight leagues three yards and a half deep, burying all the farm-houses, sugar-works, and cattle: the ashes continued falling for five days, and reached upwards of three hundred leagues from the centre of desolation and ruin.

It was at half past eleven o'clock, on the morning of the 20th, that the earthquake commenced, with an atmosphere as serene and beautiful as the elements beneath were convulsed and threatening. The first oscillation, gentle, and attended with little noise, was but the precursor of the two succeeding undulations, which were extremely violent; the duration from the first to the last vibration was about two minutes and a half, and the direction appeared to be from south-west to north-east. The sensation occasioned by the undulatory movements, seemed to me to be similar to that which would be produced by standing on a plank, the ends of which rose and fell two feet from the ground. The small streams of water which run down the streets, were checked and thrown over the edges of their channels. In Talca, eighty leagues to the south, the effects were still more violent: the oscillation commenced without being accompanied by that rumbling noise which usually is the forerunner of these awful phenomena. In Concepcion, where the great violence of the earthquake was felt, it was the second undulation which caused the havoc in the buildings; and previous to that and the many succeeding shocks, a violent report was heard, proceeding from the southward, as from a volcano in that direction. All the houses in the port of Talcahuano, which were situate on the low lands beneath the hills, were laid prostrate; and about half an hour after the vibration, when the inhabitants were returning to their houses from the heights and open spaces, it was remarked that the sea had retired so much beyond its usual limits that all the rocks and shoals in the bay were visible. It flowed again, and again retired, leaving the ships dry which were at anchor in the harbour. Then an enormous wave was seen slowly approaching the devoted town, from the direction of the Boca Chica. For ten minutes it rolled majestically on, giving time to the inhabitants to run to the heights, whence they saw the whole place swallowed up by this immense breaker.

In this moment of terror, men saw the roller with little accordance as to size; some compared it to the height of the loftiest ship, others to the height of the island of Quiriquina. It carried all before it, and rose by accurate measurement twenty-eight feet above high-water mark. A small schooner of eighty tons, nearly ready for launching, was lifted over the remains of the walls, and found lying among the ruins three hundred yards from her stocks. The reflux of this roller carried everything to the ocean. Another and a larger wave succeeded; but taking a more easterly direction, the ruins of Talcahuano escaped, but the Isla del Rey was ravaged by it. A fourth and last roller, of small dimensions, advanced, but nothing was left for further devastation. While these great waves were rushing on, two eruptions of dense smoke were observed to issue from the sea. One, in shape like a lofty tower, occurred in the offing; the other took place in the small bay of San Vicente, and after it had disappeared, a whirlpool succeeded, hollow, in shape like an inverted cone, as if the sea were pouring into a cavity of the earth. In every direction in this bay, as well as in Talcahuano, vast bubbles broke, as if an immense evolution of gas were taking place, turning the colour of the water black, and exhaling a fetid sulphureous odour.

At San Tomé, on the other side of the bay, the roller did immense damage; and on the island of Quiriquina the cattle dashed off the cliffs from panic. In this island the waves injured houses forty feet above the present level of high water, and during the three following days the sea ebbed and flowed irregularly.

In the bay of Concepcion, the strata of clay-slate have been visibly *elevated, from about three to* four feet. This alteration of the relative position of sea and land is clearly distinguishable, by a rock off the landing-place, which previous to the shock was nearly level with high water, being subsequently found to be raised three feet higher; and the buoy on the Belen Bank has now four feet less water than formerly. A vessel lying at anchor had one fathom less water alongside her than before the shock; but it is very likely that she changed her position. At the port of San Vicente, a little to the south of Talcahuano, the land has also risen about a foot and a half; and along the shore of the latter bay, even at high water, beds of dead muscles were left as proofs of the upheaval of the strata.

To the southward of the entrance of the bay of Concepcion there is a small island called Santa Maria, about seven miles long and two wide. Captain Fitzroy examined with great care the line of beach in the southern cove, as well as the northern part of the island; and from the visible evidence of beds of dead shell-fish, from soundings, and from unbiassed oral testimony, it appears placed beyond the shadow of doubt, that on the latter side the elevation of the land has not been less than ten feet, in the centre of the island about nine, and in the southern cove about eight feet. This upheaving has almost destroyed the southern port of the island, for it now affords but little shelter to vessels, and the landing is bad. Everywhere around the island the soundings have been diminished a fathom and a half, and the cliffs, of the height of 150 or 200 feet, are split and rent in all directions, and huge masses precipitated below. Both Captain Fitzroy and Captain Simpson, of the Chilian Navy, are of opinion that the uprising of the strata, both in this island and in Concepcion, at the time of the earthquake, was considerably greater, and that the many subsequent minor oscillations may have caused a subsidence to the level before recorded. At Subul, a little to the south-east of Santa Maria, the elevation of the strata appears to have been about six feet.

At Nuevo Bilbao, the port of the river Maule, seventy leagues north of Concepcion, about an hour and a half after the shock, the sea flowed above the usual water mark, and continued for half an hour in that state before a reflux took place. Fifty minutes afterwards, the sea, greatly agitated, rolled on the coast and up the river with extraordinary violence, and reached a height of twelve feet above the water mark. By this last inroad, two schooners, anchored in the port, carried away their cables, and were found among the bushes one hundred and fifty yards from the beach.

A third rush of the sea occurred half an hour afterwards, which ascended to a height of nine feet; and for the space of forty-eight hours repeated rollers came forward, but with diminished violence. No elevation of the coast has been discovered at this port, but on the bar at the mouth of the river, which has always rendered the entrance to the port both difficult and dangerous, two feet more water has been remarked; and in consequence of the fall of an immense point of a mountain into the sea, it is hoped that, owing to the new direction given to the current, no further accumulation of sand will take place.

In Valparaiso, the sea was observed to advance and recede rapidly, *but gently* and without violence.

It would be of little avail to distress the Society with the details of the ruin caused in all the southern provinces of Chile by this convulsion. To the southward of Talca scarcely a wall has been left standing, and even to the north of this line the damage caused to every description of building has been most serious. Throughout the provinces of Canqueues and Concepcion, the entire crust of the earth has been rent and shattered in every direction. In some places, fissures of several feet in depth and width have been discovered, intersecting the country for considerable distances. On one estate near Chillan, thirty leagues from the coast, extensive fissures have been the vents of muddy eruptions of salt water, which have made large deposits of a kind of gray pulverulent tufa; and on the same estate a great many circular pools were discovered of salt water, and many new thermal springs have burst forth. In many places the ground swelled like a large bubble, and then bursting, poured forth black and extraordinary fetid water.

The limits to which the oscillations extended were, to the north as far as Coquimbo, and to Mendoza on the eastern ridge of the great chain of the Andes. Vessels navigating the Pacific within a hundred miles of the coast, experienced the shock with considerable force. The bark Glenmalia, bound to Valparaiso, when ninety-five miles from the coast, and in front of the Maule, had her course through the water suddenly checked, and her rate of sailing altered from seven knots to one, and the master conceived the vessel was dragging over a sand-bank. The sea was strongly agitated, and appeared to lift the vessel twenty feet. Such was the alarm that the boats were nearly lowered: no soundings were met with.

The Island of Juan Fernandez, a mass of basalt three hundred and sixty miles from the coast, experienced the earthquake, but with less violence; the sea rose to the height of the Mole in a similar manner to that of Concepcion, and then receded, leaving the bottom of Cumberland Bay dry to some distance from the shore, and in the second rush rose fifteen feet above the usual level, carrying all before it. At the same time the Governor, Major Sutcliffe, observed a dense column of smoke issuing from the sea about a mile off the Point Bacalao, which lasted until 2 o'clock in the morning, when an immense explosion took place, which threw the water in every direction; during the rest of the night great bursts of flame rising from the same spot illuminated the whole island. Captain Simpson, about a month after, sounded near the spot in every direction, and found no bottom in less than sixty-nine fathoms. It is worthy of remark, that when on the 24th of May, 1751, the city of Concepcion was destroyed by an earthquake, and by the inroad of the sea, the rising colony of Juan Fernandez was swallowed up in a similar manner by immense rollers: the Governor, his family, and thirty-five persons perished by the catastrophe.

After the earthquake the usual atmospheric changes occurred. In many places the most awful hurricanes completed the dismay of the inhabitants, and added to the catastrophe. To these succeeded deluges of rain, a circumstance most unusual at that period of the year. At the Hot *Springs of Canqueues*, where the water issues at the temperature of *118° of Fahr., the heat* was lowered after the earthquake to 92°, a circumstance which occurred after the shock of 1822. The diminished temperature lasted but a short time.

At the risk of being tedious, I have given a detail to the Society of the changes effected in the earth's surface by this violent convulsion. After examining the extensive area of its vibration, after observing the uprising of an island and the adjacent coast, together with the eruption of a submarine volcano, it is difficult to deny that the same causes are still in operation which ages since raised tertiary formations to their present lofty site in the great range of the Cordillera. Surrounded with these continued changes on the surface of the earth, it is impossible not to respect the opinions of those philosophers who conceive that the Continent of America has risen into existence at a more modern period than that which therefore may, with more propriety, be termed the Old World.

Owing to the early hour on the 20th that the oscillation commenced, comparatively few lives were lost, but the frequent occurrence of these catastrophes, by causing organic defects, may very probably explain the causes of the short duration of human existence in these countries.

[From the *Philosophical Transactions*, for 1836.]

VOLCANIC ERUPTION IN THE BAY OF FONSECA,

ON THE WESTERN COAST OF CENTRAL AMERICA.

By Alexander Caldcleugh, Esq., F.R.S., &c.

There is, perhaps, no country on the face of the globe which shows more signs of vast geological disturbances than that part of the western hemisphere which, situate between its great northern and southern divisions, has obtained, in more modern times, the name of Central America. Its shores, extending to both oceans, are in spots precipitous, while other and extended lines of coast are low, and abound in mangrove creeks, intersected by mountains and volcanic vents, and excavated by a series of lakes, which, in the province of Nicaragua interrupt and appear to replace the great chain of the Andes. The finely comminuted scoria affords a soil which produces the richest vegetation, and a vast and new field is offered to the man of science who will boldly face the miasma of the forest, or penetrate the rich mines with which one part of the country abounds.

At the termination of a narrow promontory, which runs in a northerly direction from the southern and eastern side of the Bay of Fonseca, stands the volcanic mountain of Cosegüina, washed on three sides by the ocean, of insignificant height, and flat-topped; two eruptions are on record, namely, those of the years 1709 and 1809. Since this last date it has remained in a state of quiescence, until the period of that *stupendous* eruption on the 20th January 1835, the details of which I *now beg* permission to lay before the Royal Society. These details I have drawn up partly from official documents transmitted from the various towns to the Government of Centro-America, and partly from the information of intelligent friends, eye-witnesses of all that occurred in those days of terror. The reports to the Government, which are voluminous, fully agree on the main points: in others, probably owing to the changes of locality, and consequent variation in the direction of the wind, some slight differences are observable. It is, however, impossible to read these official reports, written too by persons little versed in classical learning, without being struck with the similarity of their description, even to the very terms he used, with that of the younger Pliny in relating to Tacitus the commencement of that eruption of Vesuvius which, nineteen centuries since, buried two cities under its ashes.

On the 19th of January, after twenty-six years of repose, a slight noise attended with smoke proceeded from the Mountain of Cosegüina. On the following morning, about half-past six o'clock, a cloud of very unusual size and shape was observed by the inhabitants of the neighbouring places to rise in the direction of this volcano. When viewed from San Antonio, about sixteen leagues to the southward, it took the appearance of an immense plume of the whitest feathers, rising with considerable velocity, and expanding in every direction. Its colour, at first of the most brilliant white, became tinged with grey, then passed into yellow, and finally became of a crimson hue. Columns of fire shot up directly through what was still imagined to be but a nebulous exhalation of extraordinary appearance; a severe shock of an earthquake was then felt. During the whole of the 20th, the cloud preserved its appearance, although unattended by that magnificence which at first predominated.

At three P.M. on the 21st, two severe shocks were felt at San Antonio and Realejo, and at midnight five others were experienced; the two first undulations were not severe, but the third and the last were terrific.

On the morning of the 22nd the sun shone brightly at San Antonio, but a line of intense darkness was observed in the direction of the cloud which had excited so much attention two days before. At the same time, a fine white ash was observed to fall around, the black line rose rapidly, the light began to fail, and darkness commenced with such quickness, that in half an hour it was more utterly dark than during the most clouded night. So intense was this darkness that men could touch without seeing each other, the cattle came in from the country showing all the signs of alarm and uneasiness, and the fowls went to roost as on the approach of night. This state of complete darkness prevailed until the following day, when at twelve o'clock it became a little brighter, and objects became visible at ten or twelve yards' distance. This atmospheric darkness prevailed two days longer, during all of which time a fine white impalpable dust continued to fall. This deposit covered the ground at San Antonio about two inches and a half, in three layers of different colours, the lower stratum being of a darker hue, the next of a grayish and the upper of a whitish appearance. For ten or twelve days a murk *light continued to prevail.*

[illegible] leagues distant [illegible] half-past [illegible] that day the [illegible] be distin- [illegible] and at five [illegible] At six [illegible] relieved. [illegible] degrees of [illegible] noises, or [illegible] clear, [illegible] hazy, and [illegible] continued to [illegible] The [illegible] of the [illegible] proceeded [illegible] every [illegible] quarter [illegible] of new [illegible] sufficiently [illegible] Conseguina, [illegible] o'clock [illegible] discharges of [illegible] con- [illegible] judgment had

[illegible] scene of [illegible] nearer. [illegible] ashes [illegible] with

[illegible] all [illegible]

promontory being extended by the ashes about 800 feet further out, gives additional reason to credit the statement.

On the 3rd of March, nearly two months after this great eruption, the volcano remained in a state of activity, but not ejecting ashes. By some geologists it has been considered that heavy eruptions of fine scoriaceous matter tend, by their falling again into the crater, to restore the volcano to a quiescent state, and that therefore this phenomenon more usually attends the conclusion of an explosion. In this particular instance it appears that the first effect of the explosion was to blow out of the crater, and finally triturate, the scoria and ashes left there twenty-six years before.

In the districts of Segovia, Comagagua, Choluteca, Nacaome, and Tegusigalpa, immense deluges of rain followed these clouds of ashes, and again gave rise to a fetid, disagreeable odour. At this season such an occurrence was extraordinary, and almost unprecedented in Central America.

I shall conclude by stating that the ashes reached as far as Chiapa to the north, upwards of 400 leagues to windward of the volcano: thus proving the existence of a counter-current of wind in the higher regions of the atmosphere. At St. Anne's, Jamaica, on the 24th and 25th of January, the sun was obscured, and not only there, but over the whole island, showers of fine ashes were observed to fall. The distance in a direct line north-easterly is about 700 miles; consequently the ashes must have travelled at the rate of about 170 miles per diem.

Captain Eden, of His Majesty's ship Conway, informs me, that in lat. 7° 26′ north, and long. 104° 45′ west, when 900 miles from the nearest land, and 1100 from the volcano, he ran forty miles through floating pumice, some of which was in pieces of considerable size.

The latitude of Cosegüina is 13° north, and longitude 87° 3′ west. Its height above the sea is computed at 500 feet.

No volcanic eruption in modern times has been recorded that reached the frightful extent of the one of which I have now had the honour of laying an account before the Royal Society. The explosion of Tomboro, in Sumbaya, in 1815, described in the Memoirs of the late lamented Sir Thomas Stamford Raffles, more nearly approaches it than any other with which I am acquainted.

[From the *Philosophical Transactions*, for 1386.]

A POPULAR COURSE OF CHEMISTRY.

VI.

GASES.——CHLORINE.

HAVING fully discussed the nature and properties of *oxygen*, and many of the manipulations in *pneumatic chemistry;* the way is in a great measure prepared, for the consideration of other *gases*, and amongst them the next that I shall mention is *chlorine*. It is a very singular *elementary body*, as will presently appear; and although I call it a *gas*, in accordance with the common parlance of the laboratory, yet, strictly speaking, it does not merit that title, and should rather be styled a *vapour*.

Now *oxygen*, as far as our present knowledge goes, is a *gas;* cool it to the lowest possible degree by the most powerful frigorific processes, or submit it to the most intense mechanical pressure known, it undergoes *no change* save that of bulk; it does not become either solid or fluid, but remains *permanently gaseous* at all temperatures and pressures. Not so does *chlorine*, under similar treatment; for, if a little aqueous vapour be present in it, when cooled down to 32° of Fahrenheit's scale, it becomes a *white crystalline solid*, or if perfectly dry, and submitted to pressure, it becomes a bright yellow *liquid;* it is, therefore, far from being permanently gaseous, and is, in fact, a true *vapour*.

The temperature of this climate is rarely so low as to alter the vaporous form of *chlorine*, and, therefore, it may be operated upon in a way somewhat analogous to that already described for *oxygen*.

Having premised thus much regarding the nature of *chlorine*, I have now to point out to you the method of extricating it from its combinations, the means of collecting it, and, lastly, some of its most remarkable properties.

The *pneumatic trough* must be filled to its proper level with *hot water*, at such a temperature that the hand can just be borne in it without inconvenience, and the bottles, or jars, destined to receive the *chlorine*, must likewise be filled with similar hot water. The glass stoppers must be greased, as already directed. *A tubulated glass retort*, holding about a wine-pint, and having a long neck, is now to be about half filled with *black oxide of manganese;* arrange it so that the *beak* or end of the neck just dips beneath the water at the hole in the shelf of the *pneumatic trough;* this is easily done if the retort is placed upon that very useful contrivance, called a *retort-stand*, the sliding-rings of which admit of ready adjustment, to a great many lengths and heights.

The proper adjustment having been made, and everything so arranged that the retort can be removed, and replaced without loss of time; remove it, and place a small glass or earthen funnel in the *tubulure*, and then quickly, but carefully, pour in as much strong *muriatic acid* as will moisten the *manganese* into a thin paste. Do not pour in the acid all at once, but a little at a time, and shake the retort round and round, so that all the *manganese* may be duly moistened; if you pour all the acid on suddenly, the chances are that it will be absorbed by the upper parts of the manganese, whilst the lower remain perfectly dry, and this would almost invariably cause the fracture of the retort, if heat was applied, and perhaps, before the operation of generating *chlorine* is finished, it will be necessary to apply a little heat: but of this presently.

Let us suppose the *acid* now properly mixed with the *manganese*, withdraw the funnel quickly, insert the stopper of the tubulure, place the retort on the retort-stand, a bottle over the hole on the shelf of the trough, and thus far we are successful. Let us proceed further.

Bubbles of some aëriform or gaseous matter, are now rising through the water into the bottle—a sort of fermentation or *effervescence* is taking place between the materials in the retort; and now its arch and neck appear of a bright *yellow* colour, this same tint is communicated to the bubbles rising in the bottle. *Chlorine* is now evolving, and it is a *yellow gas.*

The first bubbles that came over were not *yellow*, because they were not bubbles of *chlorine*, but only those of *common air*, that the retort contained; the chlorine generating behind, forced this forward.

When you judge that as much aëriform matter has passed into the bottle, as is equal to the capacity of the retort, the bottle must be slid gently on one side, and its place supplied by another, and now you see the *yellow colour* of *pure chlorine.* Remember always to make your transfer of bottles quickly, steadily, and cautiously, and to suffer as few bubbles as possible to escape into the room, for *chlorine* is a highly noxious and suffocating body, eminently hurtful to the lungs, even when very largely mixed with air: it is much the best to perform the operation in the open air, or under a shed, so that if any accidental escape of the gas takes place, the wind may waft it away before it affects the lungs.

Perhaps, in spite of all your precautions, a puff of the gas may escape, and annoy you, producing coughing, and a most disagreeable, yet indescribable, sensation in the throat, which is particularly distressing; to alleviate this, I recommend a lump of sugar soaked in weak spirits of wine, with a few drops of *sal volatile*, to be held in the mouth until it dissolves.

You have now to dispose of the impure *air* and *chlorine* in the *first* bottle: stop it under water, take it to some distance from where you are at work, and then removing the stopper, let its contents escape: attend now to the *second* bottle, which is rapidly filling with *pure chlorine;* when full, and *no water* remaining in it, stopper and remove it, place on another, always full of *hot water*, remember, and if that in the trough cools, more *hot water* must be added, so as to keep up the temperature at which you set out.

The reason why *hot water* is necessary in this process is, because *chlorine* is *absorbed* or *dissolved* by *cold water*, and if you used it, you would scarcely get a solitary bubble to rise, at all events not to remain in the bottle, but the water would become a strong *solution* of *chlorine:* this I shall speak of presently.

Now although, perhaps, *chlorine* has hitherto been evolving quietly, yet it very often happens that the materials in the retort foam up, and boil over,—or effervesce over is perhaps a more correct expression,—the neck of the retort becomes filled with a black foamy mass of the mixture, *it runs into the trough,* and soils all the water. This is very annoying, but there is no help for it; if it once happens, you had better stop work and begin again, the expense is barely worth mention—*a single farthing!* The larger the retort, the less likely is this to happen, because the materials have ample room to swell and foam in its body, without rising into the neck. A little practice will soon enable you to apportion all matters rightly.

Supposing the accident does not take place, the evolution of *chlorine* becomes sluggish after a time, and then you may excite it by the gentle heat of a spirit-lamp; but if, after continuing this heat for some time, you find a bubble or two, very suddenly evolved, with a sort of jerk, and a little water at the same instant rising in the neck of the retort, you may be sure that no more *chlorine* is coming over, but that the heat has converted part of the water of the *muriatic acid* into *steam*, which, meeting with the water in the trough, at a lower temperature than 212°, is suddenly *condensed*, and produces the *jerk*, or *concussion;* you must, therefore, stop the process, by taking away the lamp, and taking out the stopper from the *tubulure.*

If you left the stopper in, see what would happen,—why, as the retort cooled, the water from the trough would rush in and fill it, perhaps break it. This cannot happen if the stopper is out, or lightly inserted with a bit of paper or thread between the ground surfaces to admit air.

When you judge that it can be effected without risk, empty the retort, wash it well out, and set it to drain, so that it may be ready for a similar operation on a future occasion. Whenever you finish any experimenting, cleanse your vessels and apparatus, and set them by in their proper places, as soon as possible; then they will always be ready to your hands, and at the year's end, you will have saved a large proportion of valuable time, which would otherwise have been lost in hunting after things out of place, amidst a miscellaneous collection of dirty bottles, glasses, retorts, and chemicals.

Always observe the two following "golden rules."
I. "Never put off until to-morrow, that which can be done to-day."
II. "A place for everything, and everything in place."

In your laboratory you will find *shelves* far more useful and convenient than *drawers.* You have all things immediately under observation and command on *shelves*, whereas you are often tempted to cram dirty things away in *drawers;* so that the laboratory may appear neat to the casual observer. Had I my will, I would have no drawers in the laboratory.

Now let us proceed to the examination of the nature and properties of *chlorine.*

The most obvious property is its *greenish yellow* colour, and hence the derivation of the term *chlorine.* I have already said that it is injurious to life, but it supports combustion to some extent; I mean common combustion, as of a candle or taper, and of this you can easily convince yourself, by putting a lighted taper into a small bottle of it; the brilliancy of the flame is greatly diminished, it is red and smoky, very different to what happened with *oxygen.* But although the combustion of a taper is *so imperfectly* and languidly supported by chlorine, yet other substances ill burn in it with energy; and substances, too, that are not usually onsidered combustible.

For example, take the metal *antimony*, reduce some of it to a very ne *powder* in a Wedgwood mortar; place some of this on a bit of bent rd, then loosen the stopper of a bottle of *chlorine*, and throw in the *timony,* it takes fire *spontaneously*, and burns with much splendour, oducing a large quantity of white fumes. This result will not take ace in *oxygen*, and you would not have anticipated it in *chlorine*, after eing how sluggishly the flaming *hot* taper burned: how curious then is to see the *cold* metal spontaneously burst into flame! this is an instance *intense chemical affinity.*

Now observe how slight a thing will modify or prevent this affinity tween *chlorine* and *antimony.* Drop a *lump* of the metal into the gas, ere is no spontaneous combustion, no intense or immediate action: in e course of time, the *antimony* will become incrusted with a white wder, and no *chlorine* will be found in the bottle.

How is all this? why it is an instance of *mechanical aggregation* posing *chemical affinity;* the metal is one hard compact mass, strongly *gregated*, and, therefore, the *chlorine* has this to contend with and ercome before *chemical action* can ensue; it takes place *slowly* in this stance, but in the other, where the metal was in fine *powder*, aggrega- on was, to a certain extent, mechanically overcome, the *chlorine* had tle to contend with, but instantaneously exerted its action on each inute particle, *intensely* and *rapidly*, the usual attendants upon which tense *chemical affinity* are the evolution of light and heat.

Let us take another metal: we will select *copper*, in that form well own as "Dutch gold," that is in fine leaves; now slightly breathe on e end of a glass rod, about ten inches long, and cause one or two leaves "Dutch gold" to adhere to the damp surface; then open a bottle of *lorine*, and quickly plunge in the metal leaves, they instantly take fire ontaneously, and burn with a fine red light. A greenish-yellow solid bstance results from this experiment.

A small *lump* of copper, or "Dutch gold," will not burn in this anner, but is only slowly acted upon as just now stated regarding timony.

A bit of *phosphorus*, placed in a deflagrating spoon, will take fire ontaneously in *chlorine*, and burn with a pale flame, producing white mes: you remember how splendidly it burned in *oxygen;* the combus- on in this instance is far inferior in splendour, nevertheless the affinity intense.

Now let us examine the nature of the products of these three cases spontaneous combustion: I have already stated that when a metal is rned in *oxygen*, the product is styled an *oxide;* and when a similar enomenon takes place in *chlorine*, the product is styled a *chloride;* thus e first result was a *chloride of antimony*, the second a *chloride of copper*, d the third a *chloride of phosphorus;* these are all very important mbinations, but I do not enter upon their natures in detail, because it ould be out of place here, where I am only showing the general characters *chlorine.*

Cold water readily dissolves chlorine. Open a bottle of the gas, and quickly invert it in a saucer full of clean cold water, leave it for a short time, and you will gradually find the water rising in the bottle; it dissolves the gas, and the resulting solution is of a *pale lemon-colour;* chemists call it an *aqueous solution of chlorine.* If you leave any water in your bottles, of course it will absorb part of the chlorine, and produce a partial vacuum; the stoppers will become so tightly fixed as to defy all your exertions to remove them: remember, therefore, never to stop the bottles until they are *quite full of chlorine.* This *solution* has several remarkable properties: pour a little into a small glass, and then put in a bit of *gold leaf*—I mean *real gold leaf*—stir it round and round with a glass rod, the *gold dissolves*, and thus you obtain an aqueous solution of *chloride of gold.*

You would never anticipate such a result with this *noble metal*, especially if you have ever attempted to dissolve it in *nitric*, *sulphuric*, or other strong acids, for none of them have any action on it: but here is *water*, with a little *chlorine* in solution, which quickly causes the metal to disappear.

Perhaps you have heard of a *solvent* for gold, called *aqua regia*, or *nitromuriatic acid*, or if not, you can easily make an experiment which will explain its nature.

Put a little *nitric acid* into one glass, and a little *muriatic acid* into another; add a bit of *gold*-leaf to each: the acids separately have no action on it, but now suddenly mix them, and the gold instantly dissolves. You have formed *aqua regia* or *nitro-muriatic acid*, in which you find the peculiar odour of *chlorine* very perceptible; but where has it come from? Why from the *muriatic acid*, and as follows.

Muriatic acid consists of *chlorine*, and *hydrogen*, and *nitric acid* of *oxygen* and *nitrogen.* (pp. 300, 301, vol. i.) When these acids are mixed together, part of the *hydrogen* of the one unites to part of the *oxygen* of the other, forming *water*, and *chlorine* is liberated in the free state a ready solvent for gold. You recollect that we set out, by evolving *chlorine* from *muriatic acid* and *oxide of manganese;* the theory of the process, you will now easily understand.

Muriatic acid consists of *chlorine* and *hydrogen*, *oxide of manganese*, of *oxygen* and *manganese.* (p. 191, vol. ii.) When these are mixed together in the retort, part of the *hydrogen* of the *muriatic acid* unites with part of the *oxygen* of the *oxide of manganese*, forming *water*, and free *chlorine* is evolved. So you see it is a very simple case of the decomposition of the acid. There remains in the retort a solution of *chloride of manganese.* If you put all the black mass on a filter, add a little more water, the *chloride of manganese* will filter through of a pale *pink colour;* and by evaporating this solution carefully to dryness on some hot sand, or over a lamp, you will obtain a solid crystalline *chloride of manganese.*

There are many other methods of obtaining *chlorine*, especially from *common salt*, which is a *chloride of sodium;* but these are rather more complicated, and I do not stop to describe them here. Your readiest source of *chlorine* is that already detailed.

Now the *aqueous solution of chlorine* already mentioned has a very *singular* action on *vegetable colours.* Vegetable *blues* are generally rendered *red* by *acids* and *green* by alkalies. But put a bit of litmus-paper into solution of *chlorine*, and its *blue* colour is instantly *destroyed:* it is regularly *bleached*. Almost every variety of colour undergoes a similar change. Try various strips of printed calico, and you get illustrations of the *art of bleaching*.

Dissolve a small lump of *indigo* in some *sulphuric acid*, by the aid of a moderate heat, a solution is thus obtained of an intense *blue* colour, for *indigo* has the peculiar property of dissolving in *sulphuric acid*, without reddening or altering colour. Take a drop of this *sulphate of indigo*, and add it to half a pint of water, so as to dilute the *blue* a little: then pour some of it into the *solution of chlorine*, and as fast as you pour, the *blue* colour is *bleached*, with almost magical velocity. This rapid destruction of a very permanent colour is a fact of vast importance in the arts. A few years since, the *art of bleaching* was a tardy and uncertain process, dependent chiefly upon the state of the weather; but now, by the introduction of *chlorine* and its combinations, it is reduced to principles of great perfection and certainty, and enormous quantities of *chlorine* are daily evolved, and collected in a proper state, for the use of the bleacher.

In the manufacture of *chloride of lime* for the use of the bleacher, the quantity of *chlorine annually* evolved, is enormous, and almost surpasses the bounds of belief; indeed, I am almost afraid to state even its *daily* evolution by *one* manufacturer, but I had my information from very high authority. "*He evolves daily* FIFTY TONS' *weight of chlorine, and unites it with lime to make the chloride!*"

This is an enormous quantity; and a magnificent example of the perfection and precision at which chemistry has arrived.

The bleacher in most instances finds that *free gaseous chlorine*, or its *aqueous solution* are not so advantageously applicable to his processes, as *chloride of lime*. This, therefore, is the form in which *chlorine* is almost invariably employed, and *chloride of lime* is now too well known to require any further description here.

If you leave the strips of calico for some time in *aqueous solution of chlorine* or of *chloride of lime*, then take them out and dry them: you will find them very white, but very rotten, slitting and dropping into holes upon the slightest touch; because not only has the *chlorine* destroyed the chemical colour, but also the mechanical texture of the woven fabric.

This is an obvious disadvantage: how is it to be got rid of? Simply by employing a *much weaker* bleaching-solution, and then well washing the bleached goods in water until all traces of the smell of chlorine disappear. The weak solution will more slowly yet quite as effectually destroy colour, leaving texture unimpaired.

Careful manufacturers always attend to this circumstance, but others hurry their processes, and of course the goods are very rotten. "Turkey red handkerchiefs," as you very well know, are covered with *white* spots. Now in the manufacture of these articles, the piece of cotton is first wholly dyed *red*, and then *chlorine* is properly applied to those parts intended to be *white;* they become *bleached*, and it not unfrequently happens that the goods are hurried into the market retaining chlorine. All the spots and figures are, therefore, very rotten; and if you dry one of these handkerchiefs before the fire, you can readily poke your finger through all the white spots, because the fibre of the cotton is destroyed.

Paper has its dazzling whiteness conferred upon it by *chlorine*, and very frequently contains it in notable quantity, especially that paper known as "printing demy." Open a fresh bale of it, and you find the odour of *chlorine* very perceptible. Of course such paper cannot be very durable.

If you write on it with common ink, in the course of time the writing will fade; because chlorine will readily destroy the colouring-matter of *writing-ink*. It does not affect that of *printing-ink*, because that owes its blackness not to *iron* but to *charcoal*, which is a singularly unalterable and permanent substance. Blot over a printed page with common ink, and then wash it over with solution of *chlorine or chloride of lime*, which will remove all the *blots*, and leave the printing untouched. This fact is obviously of much utility.

Paper thus containing *chlorine* is often productive of the most serious inconvenience to silversmiths, jewellers, plated button-makers, cutlers, and surgical-instrument-makers, who employ it for wrapping up their polished goods; for although it appears white, and beautiful, and very neat, yet it contains *chlorine*, a sad enemy to all metal-work; the polished goods soon become tarnished, wholly spoiled, and unsaleable.

Chemistry, of course, gets sadly into disgrace, when its beautiful principles are thus perverted by the mercenary manufacturer.

In addition to the valuable property of destroying colour in the art of bleaching, *chlorine* and some of its combinations have other important uses. Thus, *solution of chlorine* or *chloride of lime* will destroy the unpleasant effluvia arising from decaying animal or vegetable matters. It is also of the greatest importance in *fumigation*, and destroys contagious or infectious matter.

People often sprinkle *vinegar* in sick chambers, or burn perfumes, and call this *fumigating;* it is of no manner of use save to *cover* a bad smell; the infectious or contagious matter, if any, remains in full activity, and it can only be effectually destroyed, by fumigation with *chlorine* or *nitric acid vapour*, the former may be depended on.

The celebrated Guyton de Morveau invented what he called "Preservative phials." These were strong glass bottles, having ground-glass stoppers, secured by a screw, and containing a mixture for evolving *chlorine.* Upon entering an infectious atmosphere, the stopper was to be slightly withdrawn, to allow a puff of the gas to escape, and its odour to become perceptible; and according to his experience, this was sufficient to avert the danger of infection.

Fumigation by *chlorine* is a very simple matter, the mixture for evolving the gas, should be placed in earthen pans on the floor of the room, the windows and doors are to be closed, and things left in this state until the evolution of *chlorine* ceases; the doors are then to be opened, and the windows also from the *outside*, letting a current of fresh air sweep into the room until you can enter without inconvenience; clear out the pans, insert fresh materials, and close up the room as before, until you judge that it is thoroughly fumigated. In fumigating large rooms, of course you must have many pans of the mixture.

Some years ago, a very extensive fumigation of the Milbank Penitentiary was conducted under the direction of Dr. Faraday: and so opious was the evolution of *chlorine*, that, upon looking through a plate f glass inserted in the door of each long gallery, the whole of its tmosphere appeared intensely yellow.

In such extensive fumigations, the operators are often dreadfully nnoyed by the accidental inhalation of the gas; and, indeed, as I have efore stated, it is most distressing. The remedy already mentioned may e resorted to, or a sponge soaked in *weak liquor ammoniæ*, may be folded n a handkerchief, and held close to the mouth, whilst operating; the *mmonia* or *volatile alkali neutralizes the chlorine*, and prevents its access o the lungs.

Chlorine is a very *heavy gas;* compared with *hydrogen*, its specific gravity is as 36 to 1, and compared with *air*, as 2.500 to 1.000, so that t may be poured from one vessel to another; in making this experiment, nd indeed, all others in which it is concerned, remember to guard gainst inhaling it.

Chlorine was discovered by the celebrated Scheele, and he called it *dephlogisticated muriatic acid*, that is, *muriatic acid deprived* of an imaginary combustible principle called *phlogiston.* The French chemists called it *oxymuriatic acid*, imagining it to be *muriatic acid* containing loosely-combined *oxygen.*

Sir H. Davy examined it with masterly skill, and found that it contained *no oxygen:* that it could not be resolved into any simpler form of matter; that it was not a *compound* but *an element*, for which he proposed the name of *chlorine*, as involving no theoretical notions regarding its nature, but simply implying its *yellow* colour; and should it at any future time be decomposed, and shown to consist of two or more bodies, still the term *chlorine* will remain unobjectionable. How different is this precision of nomenclature to that of the French school regarding *oxygen!*

If you imagine Scheele's *phlogiston* to have been *hydrogen*, you will at once perceive, how near the true elementary nature of *chlorine* he had arrived, when he called it *dephlogisticated muriatic acid.*

Now you still more fully see the absurdity of calling *oxygen* the *universal supporter of combustion*, for here is *chlorine supporting combustion*, with an equal, if not a *superior* energy; and the absurdity of the term, *universal acidifying principle*, will become fully apparent hereafter; it is slightly so at present, for *chlorine and hydrogen* produce *muriatic acid.*

Such is a slight sketch of some of the most popular properties of *chlorine*, its evolution, its power of supporting combustion, of destroying colour, and contagious or infectious matter. All these the chemist is well acquainted with, but of the *ultimate nature* of this singular substance he knows *nothing.*

THE MATHEMATICAL MISCELLANY. No. I.

CONDUCTED BY PROFESSOR GILL, FLUSHING INSTITUTE, LONG ISLAND.
NEW YORK, 1836.

It can never be an object of indifference to Englishmen, to witness the efforts made for the extension of science by their Transatlantic brethren. Those efforts have been in every sense gigantic: but especially in all that relates to the arts of life and civilization. Still, till very recently, America fell far behind ourselves in those fundamental branches of science which must form the real basis of every solid scientific structure,—the mathematical. Prior to the publication of Dr. Bowditch's translation of Laplace, and the excellent notes appended to it, the American press could not boast of one single mathematical work comparable to hundreds published in the mother-country. The cause is easily explained, and has been pointed out over and over again by American writers,—the slavish prejudice which prevails, amidst all the personal vanity of the Americans, that nothing *can be* produced in America at all comparable to the works published in London. In the arts of life they felt themselves free, and less shackled by fiscal imposts than the parent-country, and there they put forth all their strength; but in pure science, as well as in literature, they have fallen as far below us as in commercial and manufacturing efforts they have surpassed us. We are not sure, however, that the American publishers do not find it more to their advantage to keep up this delusion, than to use any effort to dispel it: inasmuch as they thus avoid the expense of purchasing "copy," but find it made ready to their hands in the form of printed books imported from London! No English work of eminence, and adapted to the taste or wants of their own population, issues from the press in this country, which is not, as if by magic, circulated throughout America, and from a dozen American presses simultaneously, in less than three months after the most favoured "town reader" has perused it! Happy the publisher who gets a single week the start of his competitors! He makes half a fortune in that single week,—provided the book is one calculated to have a "good run."

Though we are ready to admit that much of the literature, as well as of the science of America, is inferior in every point to our own, yet it must be obvious that this does not arise from want of good models,—for all that we have, they have too. It arises from the discouragement of American effort, and from this alone. Our advantage may possibly be found in this; but it certainly is not *intentionally consulted.* American patriotism would dictate a different course; and it is improbable that after the splendid efforts of Washington Irving, Webster, and Cooper, in literature, and of Bowditch in mathematical physics, that great country will look so coldly on the labours of her own children. It is her *interest* to foster the genius of her own soil; and she cannot be much longer blinded to it.

It is well known to every well-read mathematician, that in this country pure science was cultivated in comparative silence and obscurity, by means of certain periodical works more or less exclusively devoted to them (and especially the Ladies' and Gentlemen's Diaries) whilst in our own universities they lay dormant, or were taught as a mere matter of course, studied by hardly any, and considered only as matters of idle curiosity by nearly all. We live in better times: yet few of us are sensible of the great obligations which we are under to those modest and unpretending works for the present spirit of inquiry which is now become so general in respect of pure mathematical science and its innumerable applications. As we were, so America is, at this moment. Bowditch was little known to the general scientific world before his present undertaking: yet, so to speak, Bowditch was the child of Diarian nursing. His efforts were first made in those obscure American periodicals which are, except to perhaps half a dozen persons, unknown in this country, and almost as unremembered in their "father-land." Knowing this as we do, we cannot look with indifference upon the attempts which are made in that country to establish a superior class of such publications, dependent for their support on native or adopted talent. Were we to recount the names of those who in this country were *made* mathematicians by the English Diaries, Repositories, Bees, Correspondents, Companions, Receptacles, &c.,—were we to recount the names of Simpson, Landen, Dalby, Burrow, Lawson, Bonnycastle, Crakelt, Saunderson (George), Robertson (Dr. Abram), Wales, White (Thomas), Wildbore, Vince, Brinkley, Maskelyne, Hutton, Harvey, and a hundred others, already passed away,—were we to recount the names of Gregory, Leybourn, Lowry, Ivory, Wallace, Barlow, Davies, Swale, Mason, Young, Woolhouse, and a hundred others amongst the living,—we are sure the importance of these publications would be at once admitted. Can we then but augur well for America from an undertaking like this? Or, still more, from the very superior manner in which this is conducted? Can any one who peruses this single number with attention, fail to be struck with the power of American mind? We think not.

The plan of the work assimilates more with that of Professor Leybourn's *Mathematical Repository* than with any other type: and we think Professor Gill could scarcely have selected a better model, as to general feature. Should he succeed, even to a partial degree, in effecting by his publication the real good which Mr. Leybourn has done, he will deserve well of his countrymen.

We shall give a brief analysis of this number, and our readers will then see that we may most cordially recommend it to their notice and support, assured that they will find much to compensate them for its cost.

The first article is the investigation of a formula for the longitude when the rhumb is invariable. In this case, the course is the *loxodrome*, and the expression, for the longitude arrived at is known to be accurately (omitting the spheroidal figure of the earth) $x - x_1 = tan.\ \nu\ log.\ \frac{tan\ \frac{1}{2}y}{tan\ \frac{1}{2}y_1}$, where x and x_1 are the longitudes of the limits, $y\,y_1$ the polar distances of the same limits, and ν the angle of the rhumb; and the formula is well adapted for calculation*. The method has, however, been virtually anticipated by several European writers, and we may add that it is not the one most in favour amongst us.

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* We shall probably have occasion to make some remarks on the various methods which have been proposed for the solution of this problem, as to comparative facility, in a future number of this magazine.

The next is an excellent solution of the problem.—"In a given ellipse to inscribe the greatest equilateral triangle." It is shown that the triangles vary from the least to the greatest continuously; or, in other words, that there is only one maximum and one minimum value of the side; though obviously there must from the symmetrical form of the curve be *four different positions* in which they may be drawn.

Thirdly, a new discussion of the problem:—"Upon a horizontal plane a rectilineal path is traced, in which P is constrained to move uniformly. This body is connected by an inflexible and inextensible line with another body, M, which is posited in this plane, and which is supposed to have received some primitive impulse in the direction of this plane. It is required to find the nature of the curve described by the body M, and the other circumstances of the motion, abstracting all consideration of friction." This problem was first considered in connexion with several analogous ones by Clairault in *Mém. de l'Acad.*, 1736, in a discussion with Fontaine and others, who considered the curve to be the Tractory or Equitangential Curve. It has been subsequently discussed by Gergonne, Dubuat, Français, and others, (*Ann. des Math.* tom. iv.); and by Professor Lowry in Leybourn's *Repository*, vol. v., as well as in the *Ladies' Diary* for 1779 by the Rev. Charles Wildbore.

Mr. Gill obtains the equations of motion with great elegance, and differently from anything we have seen. His mode of integrating is similar to that of M. Français. He annexes the discussion of several collateral problems. This paper is on the whole a very instructive one to the young geometer and *physicien*[*].

The fourth and final article, as well as the most elaborate and important one, is a paper on *Spherical Geometry.* This is a branch of science exclusively English, and of very recent growth in any regular form. It seems to have taken a definite character from the researches of Mr. Davies, concerning the "Nature of the hour-lines on the antique sun-dials," published in the *Edinburgh Transactions*, vol. xii.; and has been followed up by two other papers on the general principles and equations which are involved in the fundamental idea. An abstract of it has also been published in the appendices to the *Ladies' Diary* for 1835—6; and several interesting questions concerning spherical loci have been interspersed through the Diaries and Leybourn's *Repository*, by Messrs. Davies, Woolhouse, and Rutherford. The abstracts above spoken of seem to have turned Mr. Gill's attention to the subject, and his processes are, in some cases, material improvements upon those of his predecessor, though, in others, they fall short of them. This article ought not to be passed over by any one who feels interested in this important branch of geometry.

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[*] Some singular discussions took place at the Bristol meeting of the British Association, respecting this term. It was *thought* "very hard that the medical *practitioner* should monopolize the term *physician,*" and after considerable altercation, the sons of Esculapius appear to have compromised this important question by allowing the cultivator of physics to adopt, after the French non-medical *savans*, the title of "*Physicien.*" Much ado, indeed, about—nothing!

We feel, however, disposed to take an exception to Mr. Gill's notation. Mr. Davies had employed the Greek* letters to designate *spherical co-ordinates*, so as to distinguish them completely from the notation of the linear system. Mr. Gill employs, contrary to the analogy which is suggested by the uniform practice of modern writers, the Italic letter. Even in plano, the angle made by the radius vector and the angular origin is written χ, θ, or ϕ; but Mr. Gill has employed y to designate the spherical radius vector, and x the polar angle. We are at a loss to account for the motives of this change; and as a *uniform* system of notation is *essential* to a successful cultivation of any branch of analytical science, we trust that Mr. Gill will yet accede to that which he found in use, except he can show that it violates some of the essential rules of a good notation, and that his own is free from that charge.

We shall conclude, by referring our readers to the remarks on the present state and probable tendency of spherical geometry to be inserted in our next number; and with cordially recommending Professor Gill's labours to the notice of our American as well as our English readers.

REMARKS ON CRYSTALLIZATION,

BY MR. THOMAS GRIFFITHS.

IN the *Journal of the Royal Institution*, (vol. xiv.,) I inserted a short notice respecting the method "of colouring alum-crystals," together with a few observations on the best nuclei for them to form on.

I there state, that "coke with a piece of lead attached to it, in order to make it sink in the solution, is the best substance for a nucleus, or if a smooth solid surface be used, it will be necessary to wind it round with cotton or worsted, otherwise no crystals will adhere to it."

This fact was afterwards noticed by Dr. Faraday, in his *Chemical Manipulation*, as follows:—"Prepare a solution of alum for crystallization by diminution of temperature; hang a thread across it, or leave in it a glass rod with a thread wound round it, and observe the greater tendency to deposition on the one substance than the other."

Now, I believe, that these are the only two published notices respecting the influence which the *mechanical texture* of a nucleus has on the deposition of crystals from solutions; I have been at work a little on the subject from time to time, and more especially, lately, in preparing illustrations for the lectures in the Chemical school of St. Bartholomew's Hospital, and have obtained several results, which, although probably already known to many experimenters, still have not hitherto been made public, as far as I know.

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* The employment of the Greek characters has a perfect analogy with the established practice of all good writers on the geometry of co-ordinates, and on physical astronomy. In truth, if the intersection of any locus, linear or superficial, with a sphere concentric with the *origin of co-ordinates be sought*, we have only to take the radius vector constant, and the polar equation of that locus (or those points, when the given locus is a line) becomes *identical with the spherical equation of the curve (or points) of intersection.* This notation is therefore in perfect keeping with pre-established ones.

I put forward no claims to originality; my object in now publishing an account of the experiments, is to furnish the lecturer, the student, and the amateur, with a few striking illustrations of some of the beautiful phenomena of the wonderful subject of crystallization.

It has often been remarked to me by some of the most eminent chemists, that for want of a record of the experiments of the lecture-table, they are either wholly lost or only verbally known, and therefore it is my intention occasionally to describe in this journal, some of the most striking experiments suitable for class-illustrations.

Experiment I. Place a smooth glass rod, and a stick of the same size, in a hot saturated solution of *alum;* upon examination the next day, the stick will be found covered with crystals, whilst the glass rod is perfectly free from them.

If I may be allowed the expression, the crystals appear to have a preference for the *fibrous surface* of the wood, it affords them good hold; they cling to it in quantity; they have none for the *smooth surface* of the *glass rod.*

When solutions are suffered to crystallize in a tall *glass* vessel, it very rarely happens that any crystals adhere to its sides, but as fast as they form at the surface of the solution, they fall to the bottom of the vessel; in a tall *wooden* vessel the case is different, for the whole of its sides and bottom become studded with crystals.

Experiment II. With a file *roughen* the surface of a glass rod at certain intervals, and then place it as a nucleus in a hot saturated solution of alum; all the crystals will adhere to the rough surfaces, leaving the smooth surfaces perfectly bright and clear.

Experiment III. Tie a few threads of lamp-cotton at certain intervals around a clean and polished glass rod, and employ it as a nucleus in a similar solution of alum; the threads will be covered with crystals, whilst the polished parts of the glass rod are perfectly free from them; and thus it is not difficult to obtain six or eight distinct bunches of crystals.

Experiment IV. Tie some threads of lamp-cotton, here and there, around a copper wire, (or a glass rod,) and then place it in a hot saturated solution of *sulphate of copper* (blue vitriol), the threads will be covered with crystals.

Coke is an excellent nucleus for alum-crystals on account of its very porous nature, affording them plenty of secure hold; but gas-coke very often has a smooth, shining, and almost metallic surface, and if a piece of it be placed in a solution of alum, it will be found that the crystals avoid the smooth surface, and form only on the most irregular and porous parts. Is it not possible that some action of this sort may be the reason why, in crystalline minerals, we so often find single, well-determined crystals, adhering to certain spots of foreign matters?

In making crystals of alum on coke nuclei, I have found it best to employ a boiling saturated solution of alum, and to bore a hole through the coke, so as to pass a string through, by means of which it may be suspended in the solution; it will of course float, and therefore the ring should be left so slack, that when the coke becomes saturated with e solution, and loaded with crystals, it may sink to about the middle the solution; this is better than loading it with lead, and the finest ystals will always be found on the lower surface, because they have rmed quietly and undisturbed by the fall of smaller crystals from the pper part of the solution.

If powdered *turmeric* be added to the hot saturated solution, the sulting crystals will be of a bright *yellow*; if *litmus* be used instead, ey will be of a bright *red*; *logwood* will yield them of a *purple*, and mmon *writing-ink* of a *black* tint; and the more muddy the solution, e *finer* will be the crystals, hence no filtration is necessary.

But in all cases of coloured alum-crystals, they are much more rittle than pure alum, and the colours are to some extent fugitive; the est way of preserving them is under a glass shade containing a saucer f water; this keeps the atmosphere constantly saturated with moisture, he crystals never get too dry, and their texture and colour undergo but ttle change. The same plan may be adopted with many other crystals, specially those of *sulphate of copper*. Those beautiful blue "crystal askets," now so common in the bazaars and toy-shops, are made with ulphate of copper, and if one of them be kept for a day or two on the nantel-shelf, it loses its beauty, becoming pale, dry, and brittle; but keep it under a glass shade with water, as just directed, and it retains its riginal beauty unimpaired.

Wire is a bad substance for a nucleus, for two reasons; first, if it is very smooth, crystals will adhere to it with great difficulty, or not at all; and secondly, supposing it has attracted crystals, they are apt to split off, on account of the expansions and contractions of the wire by alternations of temperature.

A very striking experiment showing how the *colour* of a crystal very often depends upon *water of crystallization*, consists in carefully drying a crystal of *sulphate of copper* in a crucible, until it becomes perfectly *white*; then drop it into water, and it instantly becomes of its original *blue colour* by absorbing water.

If a crystal of the *ferrocyanuret of potassium* (prussiate of potassa,) be similarly dried, its *yellow* colour vanishes, but reappears directly upon being dropped into water.

I have commenced a set of experiments on the instantaneous crystallization of concentrated solutions of Glauber's salts, in vessels secured rom the access of air; and if they tend in any way to elucidate this nysterious subject, I shall make them known through the medium of his Journal.

Chemical School, St. Bartholomew's Hospital,
October 13, 1836.

AN ELEMENTARY COURSE OF DESCRIPTIVE GEOMETRY.

I.

HISTORICAL NOTICE.

PROBABLY there are few of our readers who have not heard of the existence of a curious branch of mathematical science, called, by Monge and the modern French writers, *Géométrie Descriptive;* but those who have confined their reading to English authors, have little chance of knowing much of its nature and objects. It is true that several years ago, a considerable extract was made from Monge's work in the *Architectural Dictionary* of Mr. Peter Nicholson; but the high price and scarcity of that work preclude most readers from the opportunity of consulting it. Besides, the work of Monge, elegant as it is, is not exactly adapted for abridgment, and still less for unconnected extracts*. Mr. Nicholson afterwards commenced a work in numbers, bearing the title of *Descriptive Geometry;* but the commercial casualties of the period (1825) put a stop to the undertaking before he had entered upon the essential part of his subject; so that we have no means of judging of the method in which he intended to develop it. Nothing further on this branch of science has appeared in England.

In America, however, two separate works, on the more elementary parts of descriptive geometry, have been published for the use of the United States' Military Academy, at West Point. The first was by M. Crozet, the professor of engineering, and the other by Mr. Charles Davies, professor of mathematics in that institution. M. Crozet's work is a very indifferently selected series of extracts from Monge and Hachette; but Mr. Davies's is a work of a far higher character in its mathematical composition, though still much too confined in its scope and applications. The former was published in 1821; and was superseded by the latter in 1827; and this, again, was reprinted in 1832.

These are all the attempts which have been made to supply a treatise in our own language; and the American treatises are so little known, that we have never seen more than the single copy before us of either of them. They were all intended, too, to be subservient to *military engineering*, and hence must, of necessity, be very deficient in their applications to the wants of engineers and architects, as well as to those of the cultivators of physical science. These desiderata we hope to supply in the course of this series of papers: and we shall especially have regard to these objects, seldom taking our examples of the application of the principles of the science from other than civil or scientific pursuits, avoiding except in rare cases, the military examples of the French, Italian, German, and American writers.

Notwithstanding the importance of Geometry in the arts, as well as in philosophy, how few there are amongst those devoted to either pursuit, vho have a competent knowledge of its nature,—and how few, indeed, re those whose acquirements are of sufficient extent, to be of any real se to them! It is, indeed, a source of constant complaint, especially mongst practical men, that there is no work from which they can btain the kind of geometrical knowledge, which is adapted to their aily wants. It is true that a sufficient number of works, under various ppellations, and of various degrees of merit, have been written on the eometry of rule and compass, the construction of plane curves, and such ubjects; and also that we have excellent treatises on theoretical or peculative Geometry, besides that of Euclid: yet, on the construction of roblems relating to space, except of a purely theoretical kind on one and, or a set of artificial rules adapted to single cases on the other, we re, to the present hour, unsupplied with one single work,—a work in vhich theory and practice go hand in hand, and mutually subserve each ther. The consequence is, that the small number of persons who attend o the theory, see in it only a collection of abstract propositions, connected vith each other, and mentally beautiful in that connexion it is true, hough still having no practical bearing on the arts and necessities of ighly-civilized life: whilst the practical, who are the many that more specially require it, see only a series of isolated operations, unconnected by any common principle, and view its didactic rules as being merely so many happy contrivances discovered by accident, and resting on no other evidence than their own experience that the precepts answer their special purpose. The speculatist is satisfied with the contemplation of the truths unfolded: the practical man is satisfied so long as he finds no want beyond what his rules will help him through. Rather, we should say, this *was* the case, than *is:* as most men of science now turn their attention more or less to the utility of their inquiries; and the recent rapid strides made in every branch of the arts, shows the unavoidable and unconquerable difficulties which stand in the way of practical men, whose knowledge is not based upon theory as well as upon personal experience and traditional dogmas. There are few architects and engineers,—few even of carpenters and masons,—who have been called into an active share in executing the great undertakings that have been entered on during the last twenty years, without having felt considerably embarrassed by their want of familiarity with the higher branches of Geometry, and some degree of physical science. It is impossible to look on the great amount of labour lost, the expenses incurred, and the vexation and disappointment which have ensued,—all from the want of proper mathematical acquirement in those to whom they were intrusted,—without feeling deeply anxious to prevent, as far as possible, the recurrence of such lamentable events. We believe, indeed, that there is much truth in the statement, that "Many an excellent design has been changed to suit the workman's rule for execution, instead of the rule being extended to suit the design." Skill and taste obliged to bend to the ignorance of the carpenter and the mason! It is, on the contrary, equally true that taste is too liable to outrun the laws of nature, and to violate the rules of geometrical construction; and that many designs which are beautiful on paper, are inconsistent with the principles of equilibrium, and involve *impossible or incompatible* geometrical conditions: and the only way by which the architect or engineer can secure himself from the disappointment and chagrin of total or partial failure, is to store his mind well with the principles upon which stability is founded, and the methods of investigating those problems by which his constructions can be executed in detail. Nature is true to her own laws,—Geometry is unvarying in her principles; and the remotest consequences of the one can be followed out by means of the other. Every man, whose profession leads him to design a structure, of whatever kind it be, is false to his own reputation, as well as false to those who repose confidence in him, if he do not, for himself, ascertain, without the experiment of success or failure, whether his design be compatible with the physical and geometrical principles that pervade alike all materials and all their thousand forms. Descriptive Geometry is one of his most essential requisites in some shape or other: and the doctrine of equilibrium and of motion, is perhaps the only one which exceeds it,—if, indeed, it do exceed it*.

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* Those extracts, we believe, were made by Mr. Webster, F.G.S., the very able *geologist, who* discovered the tertiary formations in the Isle of Wight, and the author *of several* valuable papers on different geological subjects.

The gorgeous structures of Greece rather astonish us by their magnitude, and delight us by their exquisite and inimitable beauty, than by the display of mechanical and geometrical skill: whilst those of Egypt and India, from the difference of the *beau idéal* in our minds and theirs, have little to recommend them to our attention besides their antiquity and their unequalled magnitude. The "Gothic," on the contrary, is full of the most ingenious contrivance, and manifests an intimate acquaintance with the principles of strength, which was totally lost with the dispersion of that singular fraternity, under whose superintendence such structures were erected; which even the most accomplished architects of the present time have been unable to develop anew; and which the most scientific amongst us dare not venture to imitate in any structure of his own. Much as we gained by the Reformation, we also lost much! It is true we have a relic of that fraternity, in the convivial band of free-masons; and we have doubtless other relics, in the traditionary practice and rules for the simpler operations of building: but the soul is gone from that body; and can only be recalled by the assiduity of those who seek in a better spirit than has been displayed till very recently, to restore to us the knowledge which died with it.

The earliest treatise with which we are acquainted, in which any attempt beyond the most obvious geometrical operations, was made to give a body of practical instruction on subjects connected with building, was by Philibert De l'Orme, almoner to Henry the Second of France, in a work on cutting of stones, under the title of *Secrets de l'Architecture*, published in a folio volume, in 1642. Seven years later, the Jesuit Derande, and the architect Desargues, both of Lyons, published a more extensive work on the same subject; and in 1728, La Rue published a collection of *épures* for the same purpose, accurately drawn and beautifully engraved. In 1739, Frezier, *officier supérieur du génie militaire*, published in three volumes, 4to, a work entitled *Théorie et Pratique de la Coupe de Pierres et Bois*, in which he attempted to explain, by the principles of geometry, the several combinations of lines and surfaces which arise in the cutting of wood and stones: and in 1760, another very valuable treatise on analogous subjects, under the title of *Elémens de Stéréotomie*, in two volumes 8vo.

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* There is a multitude of excellent works on the first principles of statics; yet the *number* in which these principles are well applied to the wants of the engineer and architect are exceedingly few. We purpose to give a series of articles *on different*, and the most important, *of the applications* of the doctrines of *equilibrium in* our future numbers. We had prepared a paper on oblique arches with this view, but are obliged to defer it till next month, when we shall examine, with some detail, the geometrical and statical conditions of that kind of structure. For the present, all we can say to those who are projecting such arches, is,—hesitate! beware!

The military problem of *Defilement* was actively discussed by the successive professors in the Military Institution at Mézières; by Millet de Moreau in 1749, Dubuat in 1768, and Meusnier in 1777. About 1764, Monge was appointed (at the age of twenty) professor of military drawing in that celebrated institution, and he entered ardently upon the investigation of the same problem: and before he left that establishment (1784), he had published no less than seventeen *mémoires* on different subjects connected with the geometry of space, several of which were of great interest and importance, and had also framed the chief part of the work which was to give a new aspect and an unprecedented efficiency to geometrical science. It was first made public in his lectures (1794) while professor of geometry in *l'Ecole Centrale de Travaux Publics* (since so celebrated as *l'Ecole Polytechnique*), and printed for the use of the *élèves* of that school. The extreme simplicity of its mode of research, and the striking results to which it directly led, immediately gained for it not only the universal admiration of the mathematicians of that period, but the more enduring advantage of general study. In that most fickle period of all the versatile periods of French history, it might, however, have soon been thrown aside to make way for the next novelty, but for the extraordinary perseverance of the amiable and indefatigable Hachette (who had been appointed Monge's *professeur adjoint*, and became ultimately his successor alike in the professorial chair, and as a victim of Bourbon oppression), who extended its application to civil as well as military purposes, and likewise to a great number of physical inquiries. Its cultivators increased so as to include all, or nearly all, who have been any way distinguished in mathematical and physical science or the arts of life in France for the last thirty years. There is not a periodical journal on the continent, devoted to mathematics, which does not bear witness to this; and if other proof be still required, we would point to the works written on the subject by the French alone, leaving out of the account those of Italy and Germany. Hachette's own works are extremely tasteful and diversified: and those of Vallée, Léroy (to say nothing of several smaller ones, on the pure science, and those of Dupin, and a hundred others on its applications to physics and the arts,) bear ample testimony to our statement. It was introduced into Germany in 1804, if not earlier, and into Italy some years before that period: and the languages of those countries are now enriched with works of great merit on every part of the science. Even in Russia, it has, for twenty years at least, formed a standard branch of military and of a liberal education.

Still, England is to the present hour without a single work on the subject! Not *even in her military* institutions, though professedly a *military subject, is a single* lecture given on it! Not even in her universities, so strictly a branch of pure science as it is, is the very name familiar; nor does one single question occur either on the College or Senate House "Papers," that has the least reference to it! Not even amongst her practical men, whose best interests are so deeply involved in it, is its very nature and character known! Nor is this the less remarkable, when we reflect that in military works the *operations* of the method perpetually occur,—that in the geometry of co-ordinates we represent *algebraically* the data, and the conditions of the problems of descriptive geometry,—and that in every branch of the arts of life, we have occasion to employ the very *processes* of which the theoretical part of this science furnish the method and the demonstrations.

As the Geometry of Co-ordinates* is often a convenient mode of investigating the problems of Descriptive Geometry, we purpose to carry the two systems on together; and after having shown the geometrical construction, deduce also the same conclusions by means of the literal calculus. This course will, therefore, render intelligible to the minds of those who are familiar with co-ordinates the geometrical constructions of the objects with which those co-ordinate expressions are conversant; and to the minds of those who are in some degree familiar with "lines and curves in the solid," the significations of those equations by which the same ideas are expressible in symbols.

II.

Fundamental Principles and Definitions, Modes of Representation and Notation.

1. The peculiar character of the descriptive geometry consists in this: that whilst its *reasonings* are respecting geometrical magnitudes, any how situated in space, all its *constructions* are effected by lines†, situated in one plane, and having specific relations to the given magnitudes. To effect this, the method of projection is employed; both the *orthographic*, and the *scenographic* or *perspective*.

2. A point is said to be *projected* on a plane, when a line is drawn through it, in a specified manner, to cut that plane; and the point of intersection is called the *projection of that point;* and the line itself is called the *projecting line.*

3. When the line is perpendicular to the plane, the system of projection is called the *orthographic* or *rectangular*. In descriptive geometry, the *rectangular* is more frequently used than any other inclination of the projecting line.

4. The plane upon which the point is projected is called the *plane of projection.*

5. To fix the position of point, its projections upon three planes

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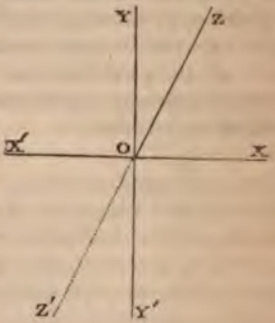
* This is generally called *Analytical Geometry,* after the French writers, who in following D'Alembert in calling algebra by the name of Analysis, have created some confusion in the minds of young students respecting the nature of analysis. As if to compensate one error by another equal and opposite one, all mathematicians have been called *Geometers.* It is desirable to be more careful in the use of terms.

† By the term *line* is to be understood not only the straight line, but *any curve-line* whatever, whose genesis is known. In popular language, the straight line alone is called in a line; but in science a line is the boundary of any surface.

be known*. No two of these planes must be parallel, in this †. In general they are so taken that each is perpendicular to the wo; and the projecting lines to one plane respectively parallel to ersection of the other two.

. Two of these planes are usually taken, the one parallel to the ı, and the other perpendicular to it. In practice, however, it will wn that one of these three vertical planes may be dispensed with; ıt one is always employed, whose intersection with the horizontal is parallel to the top and bottom of the drawing. They are the planes which, in practical works, are ed for the *plan* and *elevation*, or *sec-* The line of intersection is called the *-line.*



. In the application of algebra to the ive system, the line of intersection spoken of is usually marked x x′, the intersection perpendicular to it, in izontal plane, is marked Y Y′; and the intersection of the two vertical planes, ed Z Z′; and the three projecting lines point parallel to these three, are re- ely denoted by x, y, z. We shall de-

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have said *known*. The word *given* oyed by geometers in two senses, not always precisely agree in signi- viz., 1. to signify that which is *given* amongst the conditions of the ion; and 2. to signify those *neces- sequences* of the conditions which ılly given, and follow from them by s of construction, or reasoning, ave been previously established. general processes of geometrical , this difficulty is not felt by expe- reasoners; but it creates great ssments to young students. In ent case two projections must be ut the third can at once be deduced em, and is also, in common with erefore *known*. As, however, it imes difficult to avoid the use of hich are become part of the cur- guage of science, we may probably ently employ the word *given* in its nification on some occasions. We, e, annex the usual meanings of the ongst writers in general.

osition of a point in space is a *rela-* . Till some other data be fur- ı relation to which its position is onsidered, we cannot say that its is given: but when those are given, t is *given in position.*

metrical magnitude is said to be ıen such conditions respecting its *ıagnitude, and position* are given, *be incompatible* with any other figure different from itself, in any of these particular respects.

· A point, having position but not magnitude, is *given*, when such conditions are given as shall fix its position absolutely.

A straight-line is said to be *given in position* when two points through which it passes are given, and in *magnitude* when its length is given; and in *magnitude and position* when the two extremities of it are given.

A curve-line is said to be *given* when its mode of genesis is given, and the position and magnitude of the quantities upon which it depends, are also given.

A surface is *given* when its genesis is given, and the position and magnitude of the quantities on which they depend, are given.

In all these cases, it is obvious that by given is meant determinable rather than determined—that is, fixed, rather than actually known. Some service would be rendered to geometry by a little change in this language; especially as the same word, *given*, is also applied to those points, lines, or surfaces of reference, which are assumed as those in respect to which the others are compared.

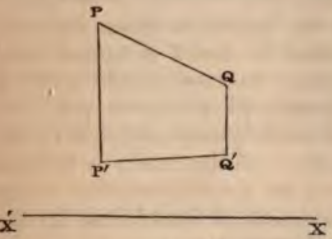
† We have said "in this system," lest the reader should suppose that the point was indeterminable when two of the planes are parallel, which would be an erroneous supposition, as they are then perfectly determinate. However the projections must be upon the two which are not parallel.

note the co-ordinate axes in the same manner in our descriptive geometry.

The three lines, XX', YY', ZZ', are in this case called *axes of co-ordinates*, *axes of reference*, or *co-ordinate axes;* the three planes are called *co-ordinate planes;* and the lengths, x, y, z, are called the *co-ordinates* of the point. In the former case, *planes of projection.*

8. The *polar*, *perspective*, or *scenographic* projection is often advantageously employed. In this case the point is projected by a line drawn through a given point, or pole, upon a given plane, and is the foundation of perspective representations. Of this we shall speak more at large hereafter, in showing the foundation of that art, and the modes of calculation by which it may also be effected.

9. It is evident that if we can project a point, we can project a second upon any planes whatever: and hence that we can project the line which joins them.



It is, moreover, easy to perceive that the projection of a straight-line upon any plane, is the same with the line which joins the projection of its extremities. For, conceive two points, situated above the plane represented by the paper, and PP', QQ', the projecting lines, and P', Q', their projections themselves. Now the two lines, PP' and QQ', being perpendicular to the same plane, are parallel to one another. (Hutton's *Course**, vol. i., cor. pr. 103, p. 339.)

But being parallel, they are in the same plane, by the definition of parallels. Hence that plane will intersect the plane of projection in a straight line, (*Course* i., pr. 89,) or the line joining $P'Q'$ is the intersection of the projecting plane with the plane passing through the projecting lines of the extremities of that line. Now the plane $PP'QQ'$ passes through the perpendicular PP', and is hence perpendicular to the plane of projection. (*Course*, pr. 110.) Hence we deduce the fact, that the projection of a given line upon a given plane is the intersection of that plane made by another plane which passes through the given line perpendicularly to the former one.

10. When the projections of a point upon two planes are given, the position of the point itself is known.

For when the projections are given, the projecting lines are also known; and as they both pass through the projected point, their intersection determines that point.

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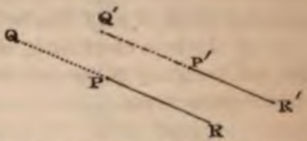
* We employ Hutton's *Course of Mathematics* as our book of reference for the theorems respecting the intersections of lines and planes, for several reasons; but chiefly that it is more concise than Euclid's, and equally satisfactory and rigid in its *demonstrations.* It, moreover, contains many other matters which will be interesting and useful to the class of readers for whom this course of papers is more immediately intended—especially a comprehensive and familiar treatise on the numerical solution of equations by continuous approximation, invented by Mr. Horner, and drawn up in the most simple form of which it appears to be susceptible. We refer exclusively to the XIth edition, edited by Dr. Gregory.

11. When two projections of a line are given, the position of the line itself is known.

For when the projections are given, the projecting planes are known, and as they both pass through the projected line, their intersection determines that line.

12. Straight-lines and planes admit of indefinite extension. They are said to be *given in position* in this case; but *in magnitude and position*, when their extremities are known, given, or determinable. When the lengths or boundaries only are known, they are said to be given in magnitude. (See note on Art. 5.)



13. When a line, whose projection on a plane is given, intersects that plane, the part before the plane, if a vertical one, or above the plane, if a horizontal one, is visible; and the parts of the projection which represent the visible is traced in full line; whilst the parts of the projection which represent the concealed parts of the line, are marked in dotted or broken lines. Thus, P R or P′R′ represents the projection of the visible, and P Q or P′ Q′ that of the hidden, part of the line, whose projection is Q R or Q′ R′.

14. We have shown in Art. 10 that two projections are sufficient for the determination of a point or straight-line, and from this it may be inferred at once, that two projections are sufficient for the determination of any curve-line, however it be situated in space, since it is sufficient for the determination of any point taken arbitrarily in that curve, and hence for all of them, or for the whole curve which is their path or locus.

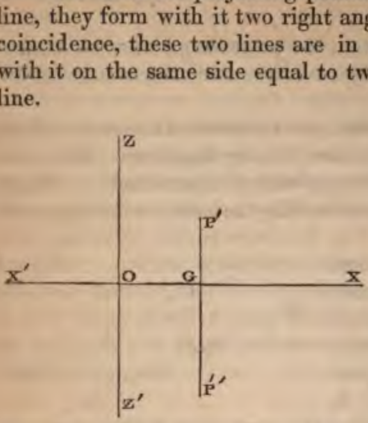
15. If a plane be drawn through the two projecting lines of any point, it will be perpendicular to the intersection of the planes of projection.

For these lines are, by the method of projection, perpendicular to the planes of projection, and hence the plane passing through them is perpendicular to those planes (*Course*, pr. 110); and hence again, perpendicular to their common section (pr. 110, cor. 3), or in other words, to the ground-line. (Art. 6.)

16. This line is called the projecting plane of the point, since it is the plane in which the projecting lines of that point are situated; and its intersection with the planes of projection are called the *projecting parallels*, since they are respectively parallel to the projecting lines, that on the vertical plane to the vertical projecting line, and that on the horizontal to the horizontal. They are called, therefore, the *vertical projecting parallel*, and the *horizontal projecting parallel.* Sometimes, simply the *vertical parallel*, and the *horizontal parallel.* They are both perpendicular to the ground-line. (*Course*, pr. 110, cor. 3.)

17. If the vertical plane of projection be made to revolve about the ground-line till it coincides with either the visible or hidden portion of the horizontal plane of projection; or the horizontal plane revolves about the ground-line till it coincides with the vertical one; then in both cases the vertical projecting parallel will coincide either by superposition or continuation, *as the case may be*, with the horizontal projecting parallel, *and they will form one straight-line*, perpendicular to the ground-line.

For both the projecting parallels being perpendicular to the ground-line, they form with it two right angles; and when the planes come into coincidence, these two lines are in the same plane, and making angles with it on the same side equal to two right angles, they form one straight-line.



18. If, therefore, we draw the ground-line $X'OX$, and in it take from O* (some fixed point from which the distances are estimated) the part OG, and draw $P'P''$ perpendicular to the ground-line; if, also, we take GP' the vertical projecting parallel, and GP'' the horizontal one; we shall then have the point P perfectly defined by the lines OG, GP' and GP''. In this, OG is the distance of the point from the plane YZ, GP' is its distance from the plane XY, and GP'' is its distance from the plane XZ. Of these we shall have rarely occasion to mention OG, and the other two are called the *horizontal distance* of the point P, and the *vertical distance* of the same point.

19. In conformity with the method of notation here employed, we shall hereafter designate the ground-line by $X'X$ (or sometimes OX), the vertical projection of points (which in their own places in space are denoted by P, Q, R, &c.,) by P', Q', R', &c., and their horizontal projections by P'', Q'', R'', &c.

20. The intersection of one plane with another, considered as being situated in the latter, is called the *trace of the former plane upon the latter*, or simply the *trace* of that plane.

21. When the traces of a plane upon the planes of projection are given, the plane itself is given; since no other plane different from this can make those traces.

22. Most of the mental and practical processes with respect to points and lines, are effected by means of these projections, whilst those respecting planes are effected by means of their traces upon the planes of projection.

23. The planes of projection, when brought into coincidence (as in art. 17,) being considered as one, we shall denominate it the *compound plane*, or *picture plane*.

Having now stated the objects we have in view, and explained the signification of the terms we employ, together with brief demonstrations of their propriety in a geometrical sense, we shall, in our next number, proceed to the solution of a few elementary problems respecting the straight-line, plane, and dihedral angle. We shall afterwards give a series of applications of these constructions to practical purposes before we enter upon the more intricate inquiries to which descriptive geometry is applicable.

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* *The line* ZZ', drawn through O in this perpendicular to the ground-line, is *identical with the trace* (next def.) of the plane which has been omitted, or that which, in *Art. 7, was* called the plane, YZ.

A POPULAR COURSE OF ASTRONOMY.

No. V.

One of the most involved and complicated problems ever proposed to the ingenuity of man, was the problem of the Heavens. A hollow concave above him, the whole of whose surface, go where he may, is apparently at the same comparatively small distance from him; the sun taking his journey across it, in a path which is not daily the same, returning day after day through some unknown region, to flood again the vast canopy of the heavens with light; stars seen in thousands at night, on this vast canopy, moving with one common motion slowly across it, between night-fall and day-break; this host of stars, different at different seasons of the year, but the same at the same season, preserving, in the *general* alteration of their position, their *relative* distances; except six of them, which wander about among the rest with a most devious motion, and are therefore called planets; the moon, too, moving with the common daily motion of the rest of the host of heaven, but, besides, revolving completely through it. every month; Winter, Spring, Summer, and Autumn, connecting themselves somehow with the variations of the daily path of the sun, and returning year after year at their appointed seasons; and eclipses of the sun and moon dependent by some inscrutable relation upon relative positions of the sun and moon;—all these things requiring, as they must have done and did, a great length of time, and much and patient observation to *discover*, constitute in their aggregate a relation of phenomena which as far surpasses any other yet unravelled by the higher researches of the human intellect, in its complication, and the vastness and dignity of the truths which it embraces, as in the simplicity of its results.

The sphere of the heavens has been hitherto spoken of as fixed and immoveable in space; and as having in its centre the EARTH,—of dimensions infinitely small and evanescent with regard to it—rolling perpetually round one of its own diameters, but never moving its centre from that of the great quiescent sphere of the visible heavens. The reader is now about to learn that this description of the position of the earth in space is incorrect:—that it does not occupy continually the same position in the centre of the sphere of the visible heavens—that its centre, and the axis within itself about which its revolution takes place, are not at rest—that these are in fact moving at the rate of about 19 miles in each second of time—that this motion is not directly forward in space, but continually round in a curve which returns into itself, and which is very nearly a circle, whose radius is ninety-five millions of miles—that nevertheless this enormous circle of the earth's revolution is itself as nothing in its dimensions compared with the dimensions of the great sphere of the visible heavens, so that the motion of the earth from the centre of that sphere, may be considered evanescent as compared with the radius of the sphere, and everything which occurs with regard to the fixed *stars, as occurring* precisely as it would occur if the earth's *centre were really quiescent* in space. Thus, then, whatever has been argued from the *appearances* of the fixed stars, on the hypothesis of the quiescence of the centre and axis of the earth in space is accurate, although this hypothesis be false.

Besides the stars called fixed, because they retain always their positions with respect to one another on the sphere of the heavens, there are other bodies visible on it, whose position does not appear to be fixed, either in reference to one another or to the fixed stars; these are the sun, moon, and planets—these, it will now be shown, lie greatly nearer to us than the fixed stars, and thus within that great sphere which has been hitherto designated the sphere of the heavens—they describe enormous ellipses in space, which are yet so small in comparison with the dimensions of that sphere, that they may be considered scarcely to deviate from its centre, and that thus, infinitely great in themselves, but infinitely small in comparison with the distance of the stars, they are all described round one common focus, very near to which lies the centre of the sun.

The process of reasoning by which the complicated apparent motions of the sun, moon, and planets, are made to resolve themselves into these few real and elementary motions, is one of the highest and most successful efforts that has ever been made by the intellect of man.

If the heavens be watched from night to night, continual alteration of the positions of the planets among the fixed stars will, from such observations, continued for a few nights, be very plainly perceived; the planet Jupiter, for instance, being seen one night in the neighbourhood of a particular fixed star, will on the next be found slightly to have receded from it, the space of a week will produce a very marked separation, a month will have taken it completely away from it, and a year will probably have carried it into some opposite quarter of the heavens—into the discussion of these apparent motions of the planets, which are very remarkable, we shall not at present enter—it is enough here to state the fact, that there are such motions, and that they do not take place irregularly and towards all parts of the heavens, but that they are, for the most part, confined to a certain zone or belt of it, about 18° in width. This zone, or belt of the heavens, is called the zodiac. A line drawn along its centre would be a great circle of the heavens, and would cut the equinoctial at an angle of 23° 28′.

The moon, too, takes her wandering solitary course eastward along this zone in the heavens. And her broad disc is continually seen covering and passing over the stars which lie along her path. Her motion, although somewhat irregular, is very rapid, being upwards of 13° in 24 sidereal hours, or nearly half a degree every hour, so that she may almost be *seen* to move among the stars.

Now the question at once suggests itself, does the sun too move, or appear to move over the concave of the heavens in which he, as well as the moon occupies a place, or does he remain in a fixed position among the stars? This question cannot be determined in reference to the sun, as we determine it of the moon—we cannot *see* the sun's motion among the *stars*, *for when the sun is up, the stars are to the naked eye invisible*;—how is it then determined? Thus:—If the sun were apparently fixed like the stars, the time intervening between the passage of the meridian of any particular place over the sun, and its return to the sun again, would evidently be precisely equal to the time of its passage over a fixed star, and its return to that fixed star again. Now this is not the case. One of these periods is called a solar, and the other a sidereal day, and the solar day is not of the same length with the sidereal day, it is always longer than it; that is, the meridian of any place on the earth's surface, always revolves from a fixed star to that star again, sooner than it revolves from the sun to the sun again. The sun then does not remain, or appear to remain, fixed like the stars, on the sphere of the heavens, it moves in the same direction in which the meridian moves, the meridian arriving at the place in the heavens where the sun was on the preceding day, before it arrives at the sun. Now the meridian revolves with the earth eastward, the apparent motion of the sun on the sphere of the heavens is therefore *eastward.*

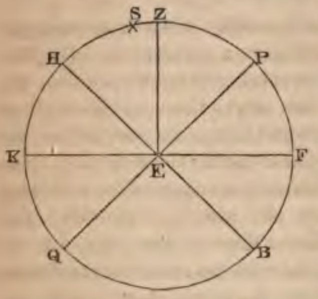
And, moreover, the amount of this daily motion of the sun eastward may readily be found; we have only to subtract a sidereal day (that is, the time which the meridian occupies in revolving from the sun on one day to the same place in the heavens on the next) from a solar day, or the time of the revolution of the meridian from the sun on one day to the place which the sun actually occupies in the heavens on the next day. The difference will be the time which the meridian has occupied in revolving from the sun's place on the preceding day to its place on this day, this difference will be found different for different days in the year, but its average is $3' \, 56\frac{1}{2}''$ of sidereal time. This, then, is the mean sidereal time which the meridian occupies in revolving from the sun's place on one day in the heavens to its place on the next day. Now the meridian revolves through the whole 360° of the heavens in 24 sidereal hours, or over 15° of it in one sidereal hour; it revolves therefore, as may readily be found by the rule of three, over an arc of $59' \, 8''$ in this $3' \, 56\frac{1}{2}''$ of time; and therefore the sun's place on the second day is $59' \, 8''$ more to the east than on the first day, or its daily motion is $59' \, 8''$ eastward. But an arc of $59' \, 8''$ being multiplied by $365\frac{1}{4}$ will give us 360°. In $365\frac{1}{4}$ days, therefore, the sun will have revolved eastward on the sphere of the heavens through 360°, that is, completely *round* it—this period is one solar year.

The sun, then, although we cannot see him moving on the heavens, there being no fixed object visible upon them when the sun can be seen to which we can refer his motion, does yet present the same phenomena as though he moved continually like the moon eastward among the stars, except that instead of completing his revolution, as the moon does, in one lunar month, his gyration takes him a whole year.

But what path does he describe in the heavens; he revolves round them, but in what route? As we cannot see him among the fixed stars, how shall we find out his course? Thus:—We may find out as is now to be shown, what declination circle he is on for every day of the year at noon, and also we may find what is his declination, that is, we may find where he is on his declination-circle. Knowing these two elements, we shall know his exact position on the sphere of the heavens, and referring to a celestial *globe or chart,* we shall tell what stars are in his *neighbourhood.*

The declination-circle on which the sun is, may be found thus:—let the exact time of the meridian passing over the sun's centre on any day be noted—now that meridian we know will return to its place in the heavens, or revolve through 360°, in 24 sidereal hours, it will therefore revolve through 180° in 12 sidereal hours. Let us then observe what stars the meridian is passing over precisely 12 sidereal hours after our first observation; we shall know that these stars are 180° from the sun's place on the preceding noon: counting, therefore, off 180° westward, on the equinoctial of a globe from that declination-circle on which are these stars, we shall know that the sun was on the declination-circle which passes through the point which we thus find on the preceding noon.

Again, to determine the declination of the sun, we have only to find, by observation, his meridional zenith distance, Z S, and subtract it from the latitude, Z H, the remainder will be the declination H S.

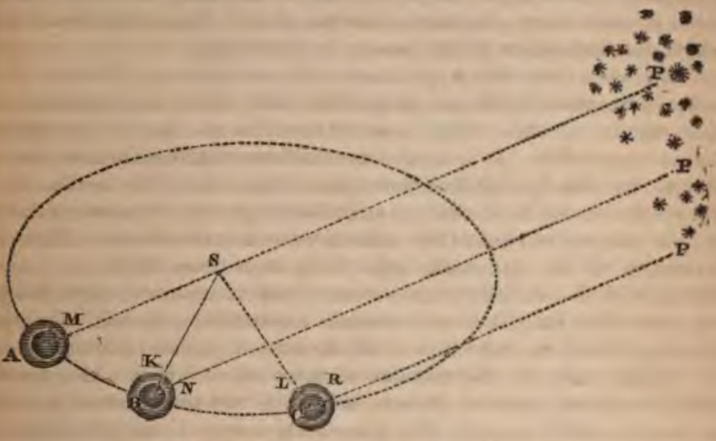


Finding thus the declination of the sun, and the position of his declination-circle for the noon of every day in the year, we can mark on the celestial globe his position for every day, and joining these positions, we shall obtain his path on the sphere of the heavens. Now, all this has been carefully done and the apparent path of the sun among the fixed stars is traced on all our celestial globes. The sun is thus ascertained to have, in common with the moon and planets, its path, called the Ecliptic, along that zone or belt of the heavens which we have called the zodiac; it is a great circle of the sphere, constituting in point of fact the *centre* of that belt which stretches 9° on either side of it. The ecliptic is inclined to the equinoctial, at such an angle, that at the greatest separation of these circles there are 23° 18′ interval between them, measured on one of the declination-circles of the sphere. Along this path in the heavens the sun would appear to us to move, as the moon does, among the fixed stars, were it not that, by the superior brilliancy of the sun, the stars are invisible to the naked eye so long as he is above the horizon.

The sun and moon both, then, have an apparent motion round the earth, the one in a year, and the other in a month. It is now to be shown that this apparent motion of the sun round the earth is not a *real* motion of the sun, but that it results from a real motion of the earth round the sun; and further, that the apparent motion of the moon *is* a real motion, resulting from an actual monthly revolution about the earth.

In the first place, then, it is asserted that a real motion of the earth round the sun would produce precisely those appearances which we observe in the heavens, and which have been attributed to an annual motion of the sun along that line which has been called the ecliptic.

Let A, B, C be positions of the earth, in an orbit which it is supposed to describe about the sun, at intervals each of a sidereal day. Also let A P, B P, C P, be lines drawn from a certain fixed star to a particular place on the earth's surface. Since the star, P, is infinitely distant, the lines A P, B P, C P, may be considered parallel. Suppose the meridian, M, of this place to be in the position A of the earth, in the act of passing over this star; then, since there is an interval of a sidereal day between the positions A and B, the meridian of the same place will be passing over the star in the position B, and similarly in the position C. Now, let S be the sun, supposed to lie about the centre of the earth's orbit, and at a finite distance from it as compared with the distance of the fixed stars. Let the small sphere described round A, as a nucleus, represent the sphere of the visible heavens in that position of the earth, or let it represent that sphere to which a spectator refers the positions of all the heavenly bodies, and on which he imagines them to be distributed. And let the spheres round B and C be similarly interpreted. At A the observer, at the place which we have supposed, will see the star P on his meridian at M[*], and the sun at the same time on the meridian; at B he will see the star at N, and the sun at K; and at C the sun will be at L, and the star at R; thus it is manifest that every time the star comes upon the meridian, the sun will appear to have receded further from it than at the preceding transit, describing arcs N K, L R, of a circle, formed by the intersection of the plane of the earth's motion with the surface of the visible heavens.



It will be observed that although the spheres to which an observer refers the positions of the heavenly bodies, and which constitute his visible heavens, are in different positions of the earth different; nevertheless, they appear to him the same, because the stars which cover them, by reason of their immense distance, do not alter their relative positions upon them. Thus, then, the sun will appear to describe a circle among the stars, which circle is, in point of fact, the intersection of the plane of the earth's orbit with that sphere of the visible heavens, which, in every one of its positions, appears to surround the observer. This circle being then supposed to be the ecliptic,—that is, the intersection of the plane of the earth's orbit with the sphere of the heavens being supposed to be the ecliptic,—all the phenomena of the apparent annual motion of the sun through this ecliptic are explained by an annual revolution of the earth about the sun in that orbit.

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[*] *He is supposed to have a telescope* powerful enough to show him the stars in the *day-time.*

Here, then, as in the case of the solar day, are two hypotheses, both of which explain the phenomena of the solar year: according to one, the sun revolves about the earth every year, in the middle of that band of the heavens called the zodiac;—according to the other, the earth revolves about the sun every year, having for the plane* of its revolution a plane which cuts the sphere of the heavens in the ecliptic.

Between these two hypotheses we have to choose. To that of the annual revolution of the sun about the earth, there are objections precisely similar to those which were adduced in the case of its apparent *diurnal* revolution. It is ninety-five millions of miles from us, and its bulk is more than one million times greater than that of the earth. Now, if it revolve round the earth in a year, this huge mass must sweep through space at the rate of about 1200 miles in every minute of time. This rapid revolution of an immensely large body about one which is in comparison with it infinitely small, at once strikes us as an improbability; it is more than this, it is a mechanical impossibility, as will plainly be perceived when we come to treat of the laws of physical astronomy.

Again, such are the motions of those other bodies which we have called wandering stars or planets, and which we shall discuss hereafter, as incontrovertibly to prove that these revolve each in an orbit about the sun; also their magnitudes may be ascertained by direct trigonometrical admeasurement, and many of them are thus found to be greatly larger than the earth. On the hypothesis we are discussing, not only, then, must the huge sun revolve continually with prodigious velocity about our earth, but the whole host of planets which accompany and revolve continually round him, and which are, many of them, as it respects magnitude, far more important elements of the material universe than our earth is, must nevertheless, together with the sun, and in combination with their motion round it, sweep with it in a perpetual circle round the earth.

It is a curious psychological fact, strongly illustrative of that waywardness and perversity of judgment of which we are all more or less the victims, that a philosopher, a laborious observer, and a man of considerable mathematical learning, was once found to take up this strange complicated hypothesis. The phenomena of the heavens were thus explained by Tycho Brahe, a Danish philosopher, the contemporary of Kepler, and the author of a very valuable catalogue of the fixed stars.

Again (to accumulate all the evidence on this point) it has been shown that the earth has a daily motion upon its axis. Now this fact of its daily motion renders also its annual motion highly probable. A probability founded on this other; that if there be two modes of explaining any phenomenon of nature, then *cæteris paribus*, that is the most probable which is the most simple. For by what we observe the creation around, we are forced upon the conviction that the lmighty acts in this respect with that economy of creative energy, hich, although infinitely more perfect in its degree, has, nevertheless, its isible type in that husbandry of *our* resources, that disposition to conomy in *our* efforts, which impels us always to avail ourselves of e simplest possible means of effecting all that we wish to do.

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* *By the plane of the revolution of the earth, is here meant a plane in which are found all the lines drawn in different posi*tions of the earth from its centre to the centre of the sun.

Thus, when, in reasoning upon any hypothesis, we are forced back pon final causes, it is sound philosophy to judge of the probability of at hypothesis according to the simplicity or complication of the final uses to which we are thus compelled ultimately to refer it. If, for stance, there be two hypotheses, by one of which we shall be compelled fall back upon a double operation of the hand of the Almighty, hereas the other resolves itself into a single effort of his will, then is e latter hypothesis, according to the analogy of nature, more probable an the former, and that INFINITELY.

Now, as has been before explained, a motion of rotation having en communicated to the earth, it must also, in consequence of the rce applied to communicate this motion, have had further a motion of anslation, unless another, or second force, had been communicated to in a direction through its centre, precisely equal to the first force, and rallel to it, but in an opposite direction; thus having as it has a motion rotation, if in other respects it be at rest, the earth must, in the ginning, have had two distinct impulses communicated to it from with- it, in opposite and parallel directions, and at different, but not opposite ints of its surface*.

Again, this hypothesis of the quiescence of the earth in space, involves cessarily the revolution of the sun about it; a *third* impulse, therefore, ust be supposed to have communicated this motion to the sun. Rea- ning, then, upon the hypothesis of the sun's annual revolution, we are liged to fall back upon three final causes, three distinct operations of e Deity, whereas the opposite hypothesis of the annual revolution of e earth subjects the whole of the phenomena to *one*. One *will*, one *pulse*, one developement of the powers of Him who spake and it was ne.

This argument (and indeed every one of those which have yet been t before the reader) is perhaps in *itself* conclusive. Different guments in proof of the revolution of the earth, have been adduced ther because it may be considered necessary to a knowledge of tronomy, that the reader should be put in possession of all that has en said on the subject, than because it is thought that the argu- ents are in any way necessary to support one another. The accumulation proofs, any one of which is sufficient, does not perhaps in reality constitute any accumulation of evidence; on the contrary, it is perhaps more according to experience, to assert that everything in the shape of proof which is added to that which is *already* proved, tends to weaken its authority. The evidence on this point is, however, too strong to be shaken by any method of arguing upon it, however illogical; yet another proof of the annual revolution of the earth will therefore now be added.

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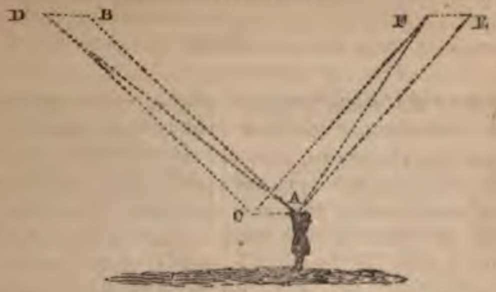
* It has been calculated by Bernouilli, at the single impulse by which the rth was made to revolve upon its axis the time which we know it to revolve, d at the same time to move forward in ace as we know it to move, must have en communicated to it in a direction erpendicular *to the line drawn from it to e sun, at a distance further from the* sun than the earth's centre is, by about the 1/165 part of the earth's radius, or at a distance about 25 miles further from the sun than the centre of the earth is. Similarly the impulse communicated to Mars must have been at a distance of 1/414 of its radius from its centre—that of Jupiter of 7/18, and that of the moon at 1/160.

Whatever may be the true explanation of the phenomena of light, certain it is, that their origin and mode of operation is subject to the usual and known laws of mechanical action. The perception of light is the effect on *impulse*, somehow or another taking effect on the nerves which belong to the retina of the eye. Now this being the case, it is manifest that the effect of that impulse may be modified by the circumstances under which the eye is placed. If, for instance, the eye be at rest, the *effect* of the impulse on the eye and the resulting perception of light, will be in the same direction as that in which the impulse is made. If it be in motion, and the velocity of its motion bear any finite relation to the velocity of the impulse of the light, then the effect on the eye, and the consequent perception of light, will not be in the direction of the impulse of the light, but in a direction compounded of the direction of the eye's motion, and the direction of the impulse of the light.

Thus, if a person standing *at rest* be struck by a ball obliquely from above, he will feel the blow in the direction in which the ball moves; but if he be in motion, the direction in which he feels the blow will be compounded of that of the ball, and that of his own motion.

We may thus ascertain what that compounded direction will be. Let us suppose a motion equal to that of the man to be communicated both to him and to the ball, at the same instant of time, but both in a direction precisely opposite to the man's actual motion—the same motion being communicated to both man and ball, the effect of the ball upon the man will not be altered by this motion thus added to it. But the man, having now impressed upon him a motion, equal and opposite to that with which he is already moving, will thus be brought to rest, and the ball will have two motions communicated to it, in virtue of which it will move in the direction of the diagonal of the parallelogram whose adjacent sides represent these motions; thus, then, its effect upon the man now brought to rest will be in the direction of that diagonal; also, by the hypothesis by which we have brought him to rest we have not altered the effect of the ball upon him; restoring, therefore, the circumstances of our first hypothesis, the effect of the ball upon the man in motion, will be in the direction of the diagonal of the parallelogram, one of whose sides represents the motion of the ball, and the other the motion of the man.

If the direction of his motion be *towards* that from which the ball comes, the effect will therefore be in a direction still more inclined downwards than the actual direction of motion of the ball, and if it be from that direction it will be more inclined upwards. Thus let him be moving in the direction AC, so as to describe AC in one second of time, and let the ball describe BA in one second of time. Then communicating *to the ball* and to the man the motion CA, equal and opposite to AC, the *man will* be brought to rest, and the ball will have the two motions CA and BA communicated to it, and will move in the direction DA. It is therefore in this direction, which is lower than its proper direction, that it actually takes effect. Similarly, if it had come from behind in the direction EA, it would have produced its effect, when combined with the motion of the man, in the direction FA, which is *higher* than its proper direction. Now for the impulse of the ball, let us substitute that of a wave of light, and let us suppose the spectator to be borne along with a velocity which has a certain finite proportion to that of the propagation of such a wave of light. The effect of the motion of the spectator on the direction in which the impulse of the light is perceived, will be precisely like that of the ball. If he is borne towards the object which is the origin of the wave, the direction in which the impulse is perceived will be lower, and if *from* it, higher than that in which it actually comes, and thus the objects towards which he is moving will appear *lower* to him, and those from which he is moving *higher* than they really are. If, then, the earth move in its orbit, and if the velocity of its motion bear any finite proportion to the velocity with which the light of the fixed stars is propagated to an observer on the earth's surface, then those towards which the earth is moving in her orbit will always appear to him lower than they really are, and those *from* which she is moving *higher*. And if she is *not* moving with any such velocity, then the light of the stars will appear to come to him in the direction in which it actually does come, and the stars will not appear higher when the earth's motion carries him from them, than when it carries him towards them. Now light travels at the rate of 192,000 miles per second, and the earth at about 19 miles per second. Thus, although the velocity of the earth bears but a small proportion to that of light, yet it does bear a certain finite and appreciable proportion. There will then be a finite and appreciable, though scarcely apparent depression, of those fixed stars towards which the earth is moving, and elevation of those in the opposite regions, provided the earth moves in its orbit as we assert; and if it do not, there will be no such depression or elevation. Now this difference of the true from the apparent position of the stars does exist; it was discovered by Dr. Bradley, and is called the *aberration of light.*



MISCELLANIES.

French Account of the Statistics of the Book-trade in Germany and England.

Germany still stands pre-eminent in the extent of its Book-trade. The annual value sold is estimated at £860,000. sterling; thirty years ago the trade was in the hands of only three hundred booksellers, or publishers. At present there are not less than 1094, including ninety-two commercial houses of Switzerland, Hungary, Prussia, and its Polish provinces. Through the Germanic Confederation there is one bookshop to 93,000 souls, while in Austria there is only one to 122,222. The progression, as regards intelligence, is still more striking in Prussia, where there is a book-shop to every 33,899 persons; in 1830, there were 200, which, in 1833, had increased to 293. At least fifty-eight new book-shops have been established in different parts of Germany between Easter, 1832, and Easter, 1833. The number of works published in that country has increased in the following proportion: (1827) 5000, (1828) 5,600, 1832, in which year many pamphlets were published) 6,122, (1833) 4,635 (?). Of these, Austria furnished 290, Prussia 1058, Saxony 1810. Leipsic is the centre of this immense commerce.

If we compare the total number of works published in Germany from 1814 to 1820, which were 50,393, and the number published in France during the same period, which was only 16,528, it would not at first be imagined that the proportional increase of literary works has been much greater in the latter than in the former country, but so it is; the aggregate amount of works was barely doubled in Germany, while in France in 1826, the number published was 4347, or four times as great as in 1814. In 1828, the French publications were 7616, a number never reached in the catalogue of the celebrated annual Leipsic fair. The fluctuations in the labours of the French press are to be attributed to political events. In 1811, *forty-five millions*, and in 1826, 144 *millions* of sheets were printed in France, the former number giving 1⅗, the latter 4½ sheets for each individual.

In England, including pamphlets, reprints, newspapers, magazines, &c., the value of printed works in 1833 amounted to £2,420,900. sterling.

This trade is almost entirely in the hands of the London booksellers, of whom there are 832, nearly as many as there are in Germany. The division of trade in this central mart of book-trading is remarkable; there are booksellers who entirely devote themselves to the sale of religious works, others to that of elementary works for instruction, and so on. Exclusive of pamphlets, reprints, and newspapers, the number of volumes published in England rose from 1105 in 1828, to 1507 in 1833, between which periods there was an annual increase of about ninety-two or ninety-three volumes, caused by the rapid progress of "cheap literature," which has effected a reduction in the mean price per work from 12*s*. to 10*s*. 7*d*.

New Surveying Instruments.

M. **Lalanne**, Engineer of the *Ponts et Chaussées*, in France, has laid before the *Académie des Sciences* three instruments for topographical surveying, which, if they accomplish all that the inventor promises, correctly and with facility, will be eagerly sought after. To the immense number of surveyors, who are about to commence operations in every part of the United Kingdom, under the numerous Railroad Acts which have passed this session, such instruments would be invaluable. They are, 1st, a Levelling Instrument, or Carriage, which it is only necessary to run over the ground, the levels of which are desired, and the section is at once obtained; 2nd, a Drawing Instrument, which lays down the plan of the ground; and can be mounted on the carriage of the Levelling Instrument; 3rd, a Power-measuring Instrument, or Dynamometer, which exhibits the effort exerted on every point of the line passed over.

Mode of ascertaining Proportion of Carbon in Cast-Iron.

ERZELIUS, in a letter communicated
M. Pelouze, to the *Société Philoma-*
ique of Paris, announces that he has
scovered a very short process of ana-
zing the various kinds of Cast-iron,
d of ascertaining the exact propor-
ns of carbon that they contain.

His mode consists of boiling the iron
a bichlorate of copper, slightly acidu-
ted with hydrochloric acid; then to
il the residue with carbonate of soda.
e weight of this second residue,
ashed and dried, gives that of the
rbon. This process has been repeated
M. Gaultier de Claubry, who found
at, to succeed, it was necessary the
lorate should be strongly acidulated
fore adding the iron in filings, other-
ise copper is precipitated; but with
is precaution, the analysis may be
mpletely accomplished in ten or twelve
inutes.

Temperature of Space.

HE result of some reflection upon the
egree of cold, registered by Captain
ack, when in the Polar regions*, in-
uced M. Arago to state to the *Aca-*
émie des Sciences, that it was his
pinion the temperature of celestial
pace could not be lower than the maxi-
num of cold mentioned by Captain B.,
iz. 102° Fahr. below the freezing-point.

M. Poinsot, on the contrary, thought
uch a consequence ought not to be
rawn from the data, and contended
hat the temperature of the upper strata
f the atmosphere must necessarily be
ower than that of space.

Amber.

"THE beautiful amber which is found on the eastern shores of England, and on the coasts of Prussia and Sicily, and which is supposed to be fossil resin, is derived from beds of lignite in the tertiary strata.

"Fragments of fossil gum were found in digging the tunnel through the London clay at Highgate, near London."—BUCKLAND's *Bridgewater Treatise* 1836.

Polarization of Heat.

M. MELLONI has addressed a memoir to the *Académie des Sciences* which contains the whole of his curious inquiries into the *Polarization of Heat*. The result, at the first view, appears very complicated, but they cease to be so on the admission, that the calorific flux from terrestrial sources is composed of rays, which, like those of solar heat, have the property of being more or less transmissible by certain solid and liquid media.

New Botanical Society.

SEVERAL meetings have recently taken place, with the intention of forming a society, to be entitled, *The Botanical Society of London.* The attendances, and promises of support by subscriptions and donations to a library and herbarium have been numerous and satisfactory. A committee is now reconsidering the laws, regulations, &c. The formation of the society is said to be patronized by Professor Lindley, and other eminent botanists.

Consumption of Oxygen by burning Wood.

MM. PETERSEN and SCHÖDLER have applied themselves to a long and most fatiguing series of experiments in order to determine the quantity of oxygen which is taken from atmospheric air in effecting the perfect combustion of a given weight of several kinds of wood.

This result has been deduced from determinations made by them, of the quantity of oxygen which each kind of wood contains before combustion, and of the quantity of additional oxygen necessary to burn it completely.

These experimenters arrived at the first of these necessary data in the following way:—Each specimen, carefully reduced to powder, was exposed in a drying apparatus, and there heated and exposed to a current of dry air until no further loss of weight occurred. It was then weighed with every precaution, and mixed with oxide of copper in a hot porcelain vessel. The mixture, after having been deprived of all hygrometric moisture in a vacuum, was burnt in a proper tube. From the water and the carbonic acid which were obtained, were deducted the carbon and the hydrogen, and the amount of oxygen contained in the wood ascertained.

Twenty-four kinds of wood were examined in these experiments. The specimens were taken, in all cases, from the trunk of the tree.

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* See p. 242, Vol. ii.

The kinds of wood, and the results obtained, are arranged, in the following Table, according to the quantity of oxygen required for their combustion.

Kind of Wood submitted to Experiment.	Quantity of oxygen that 100 parts of wood absorbs from the atmosphere during complete combustion	Quantity of Carbon contained in each kind of wood.	Quantity of Hydrogen contained in each kind of wood.	Quantity of Oxygen contained in each kind of wood.
1 Lime	140·523	49·408	6·861	43·731
2 Elm	139·408	50·186	6 425	43 384
3 Deal (white)	138·377	49 946	6·407	43 647
4 Larch	138·082	50 106	6 310	43·584
5 Horse-chestnut	138 002	49 077	6·714	44·209
6 Box	137·315	49 368	6 521	44·111
7 Maple	136·960	49·803	6 307	43·890
8 Scotch Fir (*Pinus sylvestris.*)	136 931	49 937	6·250	43 813
9 Silver Fir (*Pinus picea.*)	136·866	49·591	6·384	44·025
10 Poplar	136·628	49 699	6·312	44·989
11 Pear	135 881	49·395	6·351	48·254
12 Walnut	135·690	49 113	6 443	44·444
13 Alder	133·953	49 196	6·217	44 587
14 Willow	133 951	48·839	6 360	44·801
15 Oak	133·472	49·432	6·069	44·499
16 Apple	133 340	48 902	6·267	44·831
17 Ash	133 251	49 356	6·075	44·569
18 Birch	133·229	48 602	6·375	44·023
19 Cherry	133 139	48 824	6·276	44·900
20 Acacia	132·543	48·669	6·272	45·059
21 Beech (white)	132 312	48·533	6·301	45·166
22 Plum	132·088	49·311	5·964	44·725
23 Beech (red)	130 834	48·184	6·277	45·539
24 Ebony	128 478	49·838	5·352	44 810

The practical application of these results will be evident, if we consider that the quantities of oxygen absorbed from the atmosphere during the perfect combustion of each kind of wood, is the correct expression of the combustible value of the wood, since the quantity of heat given out during combustion is proportional to the quantities of oxygen taken up.

New Map of Central Asia.

A Map of Central Asia, compiled by M. Klaproth, and based upon communications made by the missionaries at Pekin, has been presented to the *Académie des Sciences*, by M. Landresse. By adding all the facts that could be obtained from the most recent authorities, particularly those derived from Chinese writers, M. Klaproth has been able to determine *the configuration* of the surface of these *immense countries.*

Correct Notion of Steam-Engine Horse-power.

When engineers speak of a twenty-five-horse engine, they mean one which would do the work of that number of horses *constantly* acting; but supposing that the same horses could work only eight hours in every twenty-four, there must be seventy-five horses, at least, kept to produce the effect of such an engine. The largest engine in Cornwall may, if worked to the full extent, be equal to, from a three-hundred to a three-hundred-and-fifty-horse power, and would, therefore, require a thousand horses to be kept to produce the same constant effect. In this way it has been said that an engine was of a thousand-horse-power, but this is not according to the usual computation.—*Letter of J. Taylor, Esq.. to Dr. Buckland.*

Combe.—Application of the Term.

The term *Combe*, so common in the names of upland villages, is usually applied to that unwatered portion of a valley, which forms its continuation beyond, and above, the most elevated spring that issues into it; at this point, or spring-head, the *Valley* ends, and the *Combe* begins. The convenience of water and shelter which these spring-heads afford, have usually fixed the site of the highest villages that are planted round the margin of elevated plains. —Buckland's *Bridgewater Treatise.* 1836.

Bottles, &c. sunk in the Sea.

Capt. Smyth found, on two trials, that the cylindrical air-tube, under the vane attached to Massey's Patent Log, collapsed, and was crushed quite flat, under a pressure of about three hundred fathoms. A claret-bottle, filled with air, and well corked, was burst before it descended four hundred fathoms. He also found that a bottle filled with fresh water, and corked, had the cork forced at about a hundred and eighty fathoms below the surface; in such cases, the fluid sent down is replaced by salt-water, and the cork, which had been forced in, is sometimes inverted.

Capt. Beaufort also informs me, that he has frequently sunk corked bottles in the sea, more than a hundred fathoms deep, some of them empty, and others containing a fluid; the empty bottles were sometimes crushed, at other times the cork was forced in, and the bottle returned full of sea-water. The cork of the bottles containing a fluid was uniformly forced in, and the fluid exchanged for sea-water; the cork was always returned to the neck of the bottle, sometimes, but not always, in an inverted position." — BUCKLAND, *Bridgewater Treatise*, 1836.)

Suggested Sessional Journal of the British Association.

Too much publicity cannot be given to the important investigations and undertakings which have been recommended, and for which funds have been provided, at the several sessions of the British Association, if any useful and valuable results are to be obtained from them. This publicity, it would seem, must principally depend upon the voluntary and friendly assistance of the periodical press, for, beyond the range of the General-committee-men present at the voting of the money, and who no doubt knew what they were voting it for, not the smallest effort has been made, as far as we (members) know, by the managers of the Association, to disseminate the information. One-sixth of this year has already run out, and we know individuals, selected as directors of these investigations, &c. and disposers of the funds, who are not yet acquainted with their appointment. The vital principle of the Association is voluntary exertion—scientific labour for the love of it; but how are the energies of the members and friends of the Association to be roused and directed, if the officers who possess the information do not distribute it? Suppose the reporters of the public press had not attended, and the copious accounts of the meeting had not been given by this Magazine, by the *Athenæum*, and other journals, how little would have been known of the proceedings of the Association up to this hour! We recommend the most unsparing expense to acquire, and extensively distribute, the earliest and most copious accounts of every transaction, suggestion, &c., of the Association as a body, and we submit to the consideration *of the managing* body, (if *any such exists in the interval* of the meetings,) to deliberate upon the propriety of providing reporters for each Section,—for each meeting of the General committee,—for every one of the Aggregate Meetings, that there may be obtained, and immediately printed, the most ample reports of each meeting, (even those of the Dinners should not be omitted.) Why not embody them in a morning publication, to begin a week before the meeting takes place with that preliminary information which every visiter has hitherto been distressed for, and to be continued not only during the days of meeting, but for as many days after as may bring up the arrears that unavoidably accrue? This may be entitled the "Journal of the Session," and from being prepared on the spot at the time of meeting, and under the revision of persons, in possession, from official position, of accurate and early information, it would be free from those errors and deficiencies which must happen to others who are not responsible for their accuracy, and cannot command admission to meetings, documents, &c. What grant of money which has been made, or ever can be made, would more effectually promote the great interests of the Association? What would more facilitate the acquisition and interchanges of information during the meeting, and prevent those eternal regrets of lost opportunities which fill the last day or two of the session? What could more promptly, and extensively, and satisfactorily, circulate to the distant friends of science, the gratifications, the advantages, and the acquisitions of those who are present? But, we believe, no grant, or, at most, a very small one, would be necessary. With proper management, a very low-priced *Journal* might accomplish this great desideratum, and pay its own expenses. With this on our table, we could wait with more temper than we have done during the twelve months that the *volume* is in labour.

In the absence of such means, we shall continue to acquire, and insert all information in our power, which may appear to us to be necessary for the members, and promoters of scientific information not members, to be made acquainted with. Of this nature we conceive the items in the following list to be, but of which we have no means of being quite certain of the completeness or accuracy.

Investigations, &c., recommended by the British Association—Bristol Sessi

SECTION A.—MATHEMATICS AND PHYSICS.

	Subjects.	Sums voted.	Directors.
1.	Discussion of Observations on the Tides.	£200	Lubbock.
2.	Observations on the Tides in the port of Bristol.	150	Whewell.
3.	Deduction of the Constants of the Lunar Notations.	70	Brisbane, Robison Whewell.
4.	Hourly Observations on the Barometer and Hygrometer.	30	Harris.
5.	Establishment of Meteorological Observations on a uniform Plan.	100	Harris, Phillips, Powell, Sykes.
6.	Experiments on Subterranean Temperature.		
7.	Data, depending upon very accurate measurements of points, situated on two straight lines, at right angles to each other, for the exact determination of the permanence or variability of the relative Levels of the Land and Sea.	500	Baily, Cubitt, Colby, De la Be Greenough, Grif Lubbock, Macke Portlock, Robiso Sedgwick, Steven Whewell, *Sec.*
8.	Experimental Investigation of the Form of Waves, as modified by the wind, by the depth &c., of canals, and by the manner in which the Wave is initiated.	100	Robison, Russell.
9.	Reduction of Observations in the *Histoire Céleste;* and in *Mém. Acad. des Sciences*, 1789 and 1790, tome ix.	500	Airy, Baily, Lubbock, Robiso
10.	Experiments on Vitrification.	150	Faraday, Harcou Turner.
11.	Construction of a Lens in Rock-salt.	80	Brewster.
12.	Continuation of Report on the Magnetism of the Earth.		Sabine.
13.	Report of Committee for the consideration of a proposition by Mr. Lubbock, for the Construction of new empirical Lunar Tables.		Airy, Baily, Challis, Hamilto Lubbock, Rigaud
14.	Application to the French Government for Copies of Observations on Tides.		

SECTION B.—CHEMISTRY AND MINERALOGY.

	Subjects.	Sums voted.	Directors.
15.	Inquiry into the Specific Gravity of Gases.	50	Dalton, Henry, Henry, C.
16.	Inquiry into the Quantities of Heat developed in Combustion, and in other Chemical Combinations.	30	
17.	Inquiry into the Components of Atmospheric Air.	15	Dalton.
18.	Publication of Tables of Chemical Constants.	24*l.* 13*s.*	Johnstone.
19.	Inquiry into the Comparative Strength of Iron, made with hot, and with cold, air blasts.	60	Fairbairn, Hodgk
20.	Report on the present State of Knowledge of the Chemical and Physical Properties of dimorphous Bodies.		Johnstone.
21.	Continuation of Experiments on the Effects of long-continued Heat on Minerals.		Harcourt.

SECTION C.—GEOLOGY AND GEOGRAPHY.

	Subjects.	Sums voted.	Directors.
22.	Experiments on the Quantity of Mud suspended in River-water.	20	De la Beche, Re Yates.
23.	Special Inquiry into Subterranean Temperature and Electricity.	30	Fox.
24.	Inquiry into the Origin and Nature of the Peat-mosses of Ireland.	50	Colby.
25.	Report on the Mineral Riches of Great Britain, particularly in the metalliferous districts.		Taylor.
26.	*Discovery of Plants of any kind in Slate-rocks, older than the coal-formation.*		

Section D.—Zoology and Botany.

Subjects.	Sums voted.	Directors.
. Experimental Inquiry into the Growth of Plants under glass, and excluded from air, on the plan of Mr. Ward.	25	Henslow.
. Report on the present State of Knowledge on Ichthyology.		Yarrell.

Section E.—Medicine.

Subjects.	Sums voted.	Directors.
. Investigation of the Anatomical Relations of Veins and Arteries.	50	Former Committee.
. Investigation of the Motions and Sounds of the Heart.	50	Former Committee.
. Inquiry into the Chemical Constitution of the Secreting Organs.	25	Hodgkin, Rees, Roget, Turner.
. Inquiry into the Physiological Influence of Cold in the Arctic Regions on Man and other Animals.	25	King.
. Investigation of the Effects of Poisons on the Animal Economy.	25	Hodgkin, Roupell.
. Investigation into the Pathology of the Brain and Nervous System.	25	Adams, Carmichael, Green, Macdonald, O'Beirne, Smith.
. Investigation of the Physiology of the Spinal Nerves.	25	Broughton, Cock, Harpey.

Section F.—Statistics.

Subjects.	Sums voted.	Directors.
. Inquiry into the actual State of Schools in England, considered with regard to numerical analysis only.	150	Hallam, Porter, Sykes.

Section G.—Mechanical Science.

Subjects.	Sums voted.	Directors.
Analysis of the Reports of the Duty of Steam-Engines in Cornwall.	50	Cubitt, Rennie, Taylor.
Report on the various Methods of Printing which have been proposed for the Use of the Blind.		Taylor.

Rejected by the Revising Committee.

Application to the East India Company and the Board of Control, for an accurate Census of the British Possessions in Bengal, &c.

Philanthropic Deposit.

FEEL it to be a public duty, to ke known an act of Mr. Buddle, ich will entitle him to the gratitude posterity, and has set an example ich, if generally followed, will save lives of thousands of unfortunate ners, that must otherwise perish for nt of information, which can, at this e, be easily recorded for their pre- vation. This eminent Engineer and al Viewer has presented to the Na- al History Society of Newcastle, ies of his most important plans and tions, accompanied by written docu- nts, of the under-ground workings the collieries near that town, in which those spaces are carefully noted from ence the coal has been removed. e sudden irruption of water into a ne adjacent *to such reservoirs,* is *asionally attended* with most calamitous and fatal results."—(See *History of Fossil Fuel, the Collieries and Coal Trade*, 1835, p. 249, &c.)

The dictates of humanity which prompt us to aid in the preservation of human life, no less than the economical view of rendering available, at a future time, the residuary portions of our beds of coal, which will not now repay the cost of extracting them, should induce all proprietors, and other persons connected with coal-mines, and especially engineers and coal-viewers, to leave to their successors a legacy, which will be to them precious, by preserving minute and exact records of the state of the coal in their respective districts. It can, however, scarcely be expected, that such measures will be generally and systematically adopted throughout the many coal-fields of this country, unless the subject be legislatively taken up by those official persons, whom it behoves, as guardians of the future welfare of the nation, to institute due measures, whilst the opportunities exist, for preventing that loss of life and property, which a little attention bestowed in season will preserve to posterity.—(BUCKLAND, *Bridgewater Treatise*, 1836.)

Return of Rain-Water to the Sea.

ONE THIRD only of the water which falls in rain, within the basin of the Seine, flows by that river into the sea; the remaing two-thirds either return into the atmosphere by evaporation, or go to the support of vegetable and animal life, or find their way into the sea by subterranean passages.—(ARAGO. *Annuaire, par le Bureau des Longitudes*, 1835.)

Immutability of the Nature of Light.

"WE learn from the resemblance of these most ancient organizations* to existing eyes, that the mutual relations of light to the eye, and of the eye to light, were the same at the time when crustaceans, endowed with the faculty of vision, were first placed at the bottom of the primæval seas, as at the present moment."—(BUCKLAND, *Bridgewater Treatise*, 1836.)

Valuable Acid for Engravers.

M. DELESCHAMPS has written to the *Académie des Sciences* that he has accomplished the solution of the following problem, for every kind of biting acids employed in engraving. *To obtain a clean and deep line, without sensibly enlarging the furrow in ordinary engraving, and without eating away the sides of the subject in engraving in relief.* He uses a composition of acetate of silver, and hydrate of nitrous ether. Immediately after the contact, the acetate is precipitated into the lower part of the furrow, where it produces a rapid and energetic action. The upper parts of the furrow are occupied by the nitrous ether, and preserved by its presence.

Error in the Length of the French Metre.

IN the discourse of the Baron C. Dupin on the recent progress of mathematics in France†. He noticed the correction of an intermediate part of the arc of the meridian, measured by Delambre and Méchain. It was too much to expect that a Frenchman, at such a moment, could have gone further, and stated the consequences of the error. It was an act of heroism in him to have mentioned it at all,—to have dared to point out the slightest spot on the blaze of French glory, which it was his duty to exhibit to all present. But we suppose the inflexible geometrician, who detected and honestly exposed the error, was present; and reluctant as M. Dupin must have been, to have touched a jarring chord in the feelings of such men as M M. Biot and Arago; he perhaps felt it was impossible to avoid it. He did not, however, state, as he might have done, that the error has had the effect of vitiating the correctness of the modern French unit of weight and measure,—the celebrated *Metre*.

The length assigned to this measur
by the Academy, and sanctioned b
law, is now found to be somewhat t
little. A quantity, it is true, not lar
enough to be of any consequence in t
ordinary concerns of life, but enough
be appreciable by the eye in the met
itself, and extremely annoying to m
thematicians and geometricians w
consider that as false which is n
rigorously true.

The enunciation of the error produc
as might be expected, a most liv
sensation in the Academy, particula
as two of its most eminent living me
bers, M M. Biot and Arago, had be
members of the commission intrust
with the continuation southwards of t
measurement of the arc. M. Puissa
a most able geometrician, who had h
for some years the scientific superi
tendence of the grand national map
the French territory, and author of t
Nouvelle Description Géométrique
la France, brought the subject bef
the Academy by reading a notice entitl
"A new determination of the length
the arc of the meridian, comprised
tween Montjouy and Formentera,
hibiting the incorrectness of the leng
as given in *La Base du Système métriq*
décimal."

The general triangulation of Fran
being necessarily connected with
triangles of the Dunkirk meridian, a
actually united to it by seven bas
measured by the same process, and w
the precision due to their importan

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* Trilobites.

† Page 257 of the present volume.

resented an opportunity of again comaring the bases of Melun and Perignan. In the long reticulation of riangles which separates these bases, ome are favourably disposed, and were dvantageously substituted, in the line etween the forest of St. Croix and ourges, for those of the meridian of unkirk. Now, this new comparison, o far from confirming the well-known nd, probably, accidental agreement* f these two bases, did on the contrary, xhibit a very great discrepancy; for he base of Perpignan calculated from hat of Melun differed 1 m ·82, (about ·8 English feet,) from the actual adeasurement.

This unexpected difference, which, here is now no reason to doubt, produced necessity of correcting the length of he arc of the meridian obtained by elambre, and which was employed conurrently with that of the equator in he calculation of the length of the *Metre*, seeing that this unit was defiitively settled to be 3 feet 11 lines $\frac{296}{1000}$ f the ancient iron toise of the academy aken at 13° of the thermometer of Réaumur. M. Puissant then showed, hat the true length of the arc of the meridian, which extends from the parallel of Greenwich to that of Formentera, exceeds by 90·2 toises, the length given by Delambre. Consequently, the metre ought to be lengthened the $\frac{72}{1000}$ of a line, or about the sixth of a millinetre †, in order to be precisely, as was intended, the ten millionth part of the distance from the equator to the pole.

As M M. Arago and Biot had been two of the commission for the continuation of the project, they naturally felt that the result announced by M. Puissant appeared to call the correctness of their splendid work into question. To meet the suspicion, the two academicians pointed out to their colleague that the result published by the commission had been obtained by three calculators unknown to each other, and according to a method, which necessarily introducing different combinations, as necessarily gave additional confidence on the results. At a subsequent sitting, when M. Puissant, in proceeding, stated that he now, by a mode of calculation peculiar to himself, found that an increase of 57 toises was necessary in that part of the meridional arc which crossed the triangulation of the astronomers of the commission, it was asked by M M. Arago and Biot, "whether the error lay on the side of the three calculators, or did it result from the new formula? If it could be possible that each of these three calculators could have made each separately an error of the same quantity, was it not also possible that this newly-introduced formula might not be quite exact? And," say M M. Arago and Biot, "as the works relative to the determination of the metre are not even yet entirely published, nor definitively settled, we shall again undertake this subject, and then we will scrupulously examine the methods employed in the calcution of our triangles, and on whichever side shall fall the error of the actual computations, we shall not hesitate to point it out." M. Puissant, by no means disposed to accept this promise of future correction, continued to agitate the question with his scientific colleagues, and after having gone through the calculations by two different methods, and by means of tables of geodæsical measures, his first conclusion was confirmed; and he asserted that there was an error of 57 toises, (about 121¼ English yards,) which affected the true meridional distance as calculated by the *Bureau des Longitudes*, between Montjouy, and Formentera,—"an error," said M. Puissant, "which it is important to the interests of science to correct, and which can only be explained by supposing some mistake between the two stations at Morella, which are a very small distance from each other, and were required to connect Montjouy with the general triangulation."

Subsequent investigations, made by the indefatigable M. Puissant, into every part of his previous labour, and into every document on which his calculations were founded, and additional repetitions of his calculations, not only established the reality of the error, and of its amount, but also enabled him to point out the actual position of the place where it occurred. He ascertained that it ought not to be attributed to Delambre nor to any of the scientific successors to him, in that part of the arc measured by them, but that it proceeded from the circumstance, that the station of the second order, fixed by Méchain at Sierra Morella, has, inadvertently, been taken for that which he had used, at the same place, to form the triangle Matas-Montjouy-Sierra-Morella, (the first of the Table, p. 179, Vol. IV., of *La Base du Système Métrique*.) M. Puissant has, in consequence, assigned to the arc in question a length of 153662·75 toises, and not that of 153605·2, given to it by MM. Bouvard, Mathieu, and Burckhard.

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* "On this point," a French writer says, "an incontestable proof of the accuracy of these observations is, that the base of Perpignan, calculated from that of Melun by the chain of triangles which connects them, differs but 10 or 11 inches (French) from the actual admeasurement of the latter, though the interval which separates them is more than 700,000 metres," (about 435 English *miles.)*

† *Equal about* $\frac{1}{152}$ *of an English inch.*

Though it is to be lamented that an error, of the smallest possible account, should have occurred in so important a work, yet it will not be without some good effects; it will establish the necessity of the increasing vigilance and uncompromising rigour, so ably urged by M. Dupin, in his Discourse before the Academy[*], as imperiously demanded in all such operations; and, piqued as the surviving members of the commission must be, it will enlist all the personal feelings and gigantic acquirements on the side of further investigation, so that, eventually, the length of the metre will be determined to the greatest possible minuteness, and without any chance of future correction ever being found to be necessary.

The method, by the aid of which M. Puissant has determined the length of an arc of the meridian, is based upon the most indisputable principles, and the various applications he has made of it have demonstrated its simplicity and correctness. In using it for the purpose referred to, he ascertained that the depression of the terrestrial spheroid, generally taken at $\frac{1}{309}$, is only $\frac{1}{306}$, an amount precisely the same as that deduced from the lunar inequalities.

Blissful Ignorance.

The ladies of North Britain and others, admirers of the beetle-stone, and who frequently select it from their trinket-box, and place it in a conspicuous situation on the person, in the shape of a brooch, &c., will hardly thank their facetious friend, Dr. Buckland, for the following account of the origin of the jewel:

"In 1832, Mr. W. O. Trevelyan recognised coprolites[†] in the centre of the nodules of clay-ironstone, that abound in a low cliff composed of strata belonging to the coal-formation at Newhaven, near Leith. I visited the spot with this gentleman and Lord Greenock, in September, 1834, and found these nodules strewed so thickly upon the shore, that a few minutes sufficed to collect more specimens than I could carry; many of these contained a fossil fish, or fragment of a plant, but the greater number had for their nucleus a coprolite, exhibiting an internal spiral structure; they were probably derived from voracious fishes, whose bones are found in the same stratum. These nodules take a beautiful polish, and have been applied by the lapidaries of Edinburgh to make tables, letter-pressers, and ladies' ornaments, under the name of beetle-stones, from their supposed insect origin."

In spite of their polish and their beauty, beetle-stones will now, we fear, be irrevocably banished from the toilet and the writing-table. Alas!

> "Where ignorance is bliss,
> 'Tis folly to be wise."

Cold fatal to Barbel.

M. **Agassiz** has observed that a sudden depression to the amount of 15°, of the temperature of the water in the Glat, which falls into the lake of Zurich, caused the immediate death of thousands of barbel.

First Belgian Scientific Congress.

The first national association of scientific and learned men, in Belgium, met at Liège, on the first of August last. About 130 members had entered their names at the commencement of the session; among them were those of two ladies, one a botanist, the other a poetess. M. Caumont, of Caen, the founder of the scientific congresses of France, was elected president. The business of the congress was, as in other similar national associations, divided into sections: but the subjects of inquiry were of far greater variety than in the German or the British meetings. At Liège there were added to the list, legislation, social economy, agriculture, manufactures, commerce, history, archæology, philology, and literature.

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[*] See page 257 of this Volume.

[†] Fæcal remains, in a state of petrifaction, dispersed through the same strata in which the skeletons of some fossil animals are buried.

The state of preservation of these very curious petrified bodies is often so perfect as to indicate not only the food of the animals from which they were derived, but also the dimensions, form, and structure of their stomach and intestinal canal. (See *Trans. Geol. Society*, London, 1829.) Buckland, Bridgewater Treatise, 1836.

Another distinct feature of the Belgian congress was, the preparation of questions on points considered the most desirable to be elucidated. These amounted to about 80: they were printed in a programme, and distributed. At the general meetings they were proposed *seriatim*, and several of them gave rise to discussions of great interest, and pregnant with the most valuable information. When it is stated these discussions did not prevent papers, of great interest, being read at the general assemblies, it will puzzle the visiters of the Bristol session of the British Association, to imagine how so much business could be accomplished. It may be useful that they should know that the Belgian philosophers met in section at *six o'clock in the morning*, and worked till two. They then rested and refreshed, and, at four in the afternoon, met in general assembly. If we suppose, as we reasonably may, from the business done, that the *Bureaux* were attentive, the chairmen exact to the appointed hour, and the members punctual, what a contrast shall we have to the blank days, the late hours*, the empty committee-rooms, and the vacant chairs, which were so frequently *complained* of at Bristol!

One very remarkable circumstance occurred, and, it is said, commanded the deepest attention of the congress: a Miss Anna Knight†, niece of Mr. William Allen, delivered an address on the abolition of the slave-trade, and on the complete emancipation of the negroes.

The second Belgian congress is to be held at Ghent, in September, 1837.

Bug-destroying made easy.

M. Fournel, after having in vain attempted, by every known means, to destroy the bugs which infested a bedstead, happened, accidentally, to place in the room where the bedstead stood, a handful of the roots of the narrow-leaved pepper-wort, (*Lepidium ruderale.*) A short time after, he was greatly surprised, to find the bugs had entirely disappeared. On examination, he perceived that the insects had attached themselves in prodigious quantities to every branch, leaf, and flower of the pepper-wort. The fact is of great importance to domestic comfort, as it furnishes an easy means of attracting these insects from their generally inaccessible retreats, and consequently of effectually extirpating them.

Application of Science to Navigation—French Prize-question.

During the time that the Baron C. Dupin held the office of Minister of the Marine, in the government of France, he induced the king to offer a reward of 6000 francs (240*l.*) for "*The work or memoir, in which the application of the mathematical sciences to the art of navigation shall be carried to the farthest extent.*

The term prescribed for the receipt of the competing works expires in the ensuing month. The decision on their merits is to be made by the *Bureau des Longitudes.*

New Theodolite for surveying underground.

M. Combes, professor at the *Ecole des Mines*, has supplied a deficiency felt by mining engineers, of an instrument for subterranean surveying; it is an adaptation to this express purpose of Gambey's theodolite, which was constructed for the French national geodæsic operations. The minutiæ of the construction of M. Combes's *Subterranean Theodolite*, as he has termed it, would not be intelligible without a figure; and we notice the improvement merely to apprize our countrymen of its existence.

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* The sectional committees at Bristol rarely met before half-past ten, and it was seldom that a section was in actual operation till eleven, frequently much later, and, except in one or two of them, "school was up" often before three. This, with "half-day holidays," on Tuesday, Thursday, and Saturday, made a most serious waste of time.

† We should think this is, perhaps, an error, and that it was the *ci-devant* Miss Frances Wright (now married to a French gentleman, and resident at Paris), the lady who made such heroic efforts in this cause *in the United States, that* addressed the congress.

Recent Account of the African Desert Sahara.

THE extent, nature, formation, and encroachments of this mighty desert have lately been more accurately defined than heretofore by Ritter, in his *Géographie Comparée.* It is bounded on the north by the Biledulgerid (*the Country of Dates*); to the east it reaches the Atlantic; towards the south it stretches, very uniformly, to the Senegal and the Niger, with which rivers it extends between the 16° and 18° of north latitude, into the unknown regions of eastern Soudan. In magnitude, the Sahara may be compared to the half of Europe, or still more definitely to about double the Mediterranean; the area of the desert being 72,000 geographical square miles, including the Oases, and to 50,000 square miles without them: its length is 450, and its width 300 geographical miles.

The essential characteristic of this desert consists in its *uniformity* both of surface and substance; the dead level of the former is only varied by comparatively slight elevations and depressions,—a feature which precludes the accumulation of large bodies of water,—while the latter, generally speaking, is a mass of pebbles, or salt, equally distributed. The limestone, which rises into the elevations in the neighbourhood of Fezzan, and in the Haroush, is, according to Humboldt, of a formation analogous to that of the Jura: in Darfour it occasionally encloses granite, talc, and basalt. The rocks are covered over with pebbles, shingle, and with moving sand, which the wind raises in clouds like to a fine fog. The sand of the Libyan desert is composed of transparent fragments of quartz, about the third of a line in diameter; with this the surface of the subjacent rock is equally covered over in a manner analogous to that with which snow would be deposited.

The eastern portion of this vast desert is much freer from sand than the western; it is intersected by low rocky shelves, denuded of sand, and quite barren, and it contains a great number of oases. The western part, on the contrary, is nearly destitute of the latter; and the few that are met with are very limited in extent. The terrific hurricanes, which about the period of the equinoxes, annually sweep over this *sandy ocean,* being directed from east to west, necessarily lay bare the eastern portion. Hence arises that great quantity of rolled pebbles, of naked rocks, and of denuded oases, which are found in the *Sahara,* or eastern district; while the moving sands of the western one, (the *Sahel*) advance gradually towards the ocean, and are formed on its shores into sandy downs by the effect of the great rotatory movement of the Atlantic Ocean.

Excellent springs, wells, marshes, and lakes, are met with in the Sahara, on the sides of the ranges of rocks, especially in Winter: but all these advantages disappear in the Sahel, where nature presents no variety. Rivers, springs, oases, wells, and saline lakes, are there nearly unknown. Nothing is seen but indurated rock-salt, and moving sand: water is only to be met with by digging to an enormous depth, even where the process is at all practicable; while the few wells which are dispersed over the interior, are so deep that the caravans can derive no advantage from them. To prevent their being choked with sand, the wells which have been successfully established are walled round with bones of camels for want of stones, and covered over with skins: the skill with which the conductors of caravans trace their way in these monotonous solitudes to these wells when once known, is admirable.

The prevailing winds in the Libyan desert are from the north and north-east; the consequence is that hills of sand are continually advancing from those quarters, at the usual rate of ten or twelve feet, as has been estimated by the gradual disappearance of springs and wells: these winds only raise into the air the fine sand;—the pebbles and shingle remain bare. The moving desert consequently covers with its sands those spaces which it has gained, while the wind from the district of moving sands converts it into a plain of gravel, pebbles, and shingle. The portion designated as Sahel, thus forming the advance guard of the Sahara, in time becomes converted into the latter; but this phenomenon is not general in Libya. The progressive advance of the sand does not then leave behind it, as might be expected, vast naked plains, because the Mediterranean is ceaselessly throwing up considerable masses of sand on its shores, these the winds seize hold of, and carry towards the interior of the continent: this process may be detected by the gradual disappearance of the palm-trees under the sand along the coast. The sea has even, in many places, enabled the Libyan desert to extend its domain at the expense of the valley of the Nile. This encroachment is especially remarkable near to the village of Warden, at the northern extremity of the plain of Gizeh. A belief in this encroachment of the sands is general among the Arabs, who possess accurate knowledge of all the phenomena of the desert. The well-known colossal sphinx which is now half buried by the sand, appears to them a talisman, incessantly conjuring the storm of sand not to advance further. Caviglia, who undertook to excavate the hillocks of sand accumulated at its base, discovered many remarkable objects which the desert had ingulfed.

The same effects from the sand are perceivable in Nubia, whereof the numerous sphinxes, forming the avenue leading to the propylæum of the temple of Sibhoi, six alone are now visible: all the others, together with the greater part of this superb temple, being overwhelmed by sand.

An account of a recent personal visit to the Sahara is about to issue from the French press at Algiers. M. Baudoin, a native of Provence, being at Algiers soon after the taking of the city by the French, was surprised, as he was walking in the environs, by a party of Arabs of the Issers tribe, and carried off by them into their own territory; there they circumcised and sold him to a Mahometan priest. A long series of travels and singular adventures followed this event, and with his master he reached the Sahara. During his journey, he describes having seen some wealthy cities and interesting ruins; among the latter he saw inscriptions, whose characters resembled neither the Greek, nor Latin, nor Arabic alphabets. Having obtained his liberty by the death of his master, who fell a victim to the cholera, which raged to the very centre of Africa in 1835, he immediately prepared to rejoin his compatriots, and succeeded by avoiding the direct routes. The narrative, by a competent person, of a journey made under such circumstances, would be extremely valuable, as the part of Africa said to have been visited *is scarcely known to Europeans.*

Huttonian Theory of Rain Controverted.

The most plausible Theory of Rain ever given to the world, is that of Dr. Hutton. He supposes two currents of air of different temperatures, both nearly saturated with vapour, to be mingled together, and that a precipitation of course takes place, in accordance with the known fact, that at their mean temperature all their vapour cannot be retained, and therefore the surplus will be precipitated. This theory is defective in two respects: First, it does not show how two currents of air could be mingled to any considerable extent; and second, it does not show by calculation, that rain to any considerable amount would be produced, even if large masses of air, at very different temperatures, should be mingled together, which it would be easy to show never can happen, especially in the torrid zone. It may fairly be presumed that no advocate of the Huttorian theory would suppose that more than five hundred feet of a stratum of cold air could be mingled with a stratum of warm air, five hundred feet of perpendicular height. Now it will be found by calculation, that if one of these strata is at 60°, and the other at 40°, and both saturated previous to their mixture, the whole amount of precipitation, provided they took the mean temperature of 50°, would be less than a grain and one half on each square inch of surface. But as the latent caloric evolved in the condensation of the vapour, would not suffer the mean temperature of the two strata, when mixed, to be acquired, but some temperature above 50°, therefore a less quantity than that mentioned would be precipitated. Such a quantity, in most cases, would be entirely evaporated in passing down through the air below, and never reach the earth.

It was mentioned before, that 5·1 inches of rain fell in Wilmington, on the 29th of July, 1834, in two and a half hours; let us see whether such a rain could be produced at all, on the Huttonian principles, making the most extravagant allowance for the quantity of air mingled, and also for the difference of temperature of the two strata.

Let us suppose, then, that one-half of the atmosphere at 80° Fahr., should be mingled with the other half at zer over the region round Wilmington, a that 5·1 inches of rain is the result. What will be the temperature of the mingled mass after the rain? The mean temperature is 40°, which would be the temperature after the mixture, if no latent caloric is given out in the condensation of vapour. But from the principles explained before, it will be found, that as five inches of rain is $\frac{5}{400}$ of the whole atmosphere in weight, the latent caloric given out in the condensation of the vapour forming this rain, will be sufficient to heat the whole compound 59·7°, which being added to the mean temperature 40°, will make the temperature of the air after the rain 99·7°, almost 20° hotter than the hottest half of the atmosphere before the mixture.

Having found that the Huttonian theory would not bear the test of calculation, I imagined there was but one other possible mode of condensing vapour, and that was that the vapour by its own elasticity in the lower parts of the atmosphere, thrust itself up into a cold stratum above, whenever such a one overlapped the one below, and was thus condensed into rain.

This hypothesis, I thought, was altogether reasonable, from the great discovery of Dalton and Gay Lussac, that vapour in the atmosphere rests only on vapour, and thus forms an independent atmosphere, and is not supported in the least degree by the air. I imagined, then, that vapour could rush with great velocity from air where the dew-point was high, to air where the dew-point was low. But when I discovered that some rains were so great as to be beyond the power of this theory, too, I began to suspect the hypothesis itself, which induced me to put it to the following trial.

I united two glass retorts together by their necks, then having covered one with snow, I put ten drops of water into the other, and placed it in a vessel of water at the temperature of 130°, letting it remain in that situation seven hours, the temperature of the room during the experiment being about 70°; not one drop was distilled over in all that time.

I then took the retorts apart, leaving open the neck of the one having the water in it; it has continued in the room, open now for thirty days, with a temperature of 70° night and day, and the *dew-point* in the room never as high as *40°, the ten* drops of water being now *only slightly* diminished.

This refutes the hypothesis of rapid permeation of air by vapour, and, indeed, proves that vapour, like heat, when it passes up to the upper regions, must be carried by the air, and not thrust up by its own elasticity. But to return from this digression; if the Huttonian theory is unable to produce such a rain as that at Wilmington, what will it do with the one which occurred at Geneva, on the 25th of October, 1822, when it rained thirty inches in twenty-four hours; or the one at Joyeuse, on the 9th of October, 1827, when it rained thirty-one inches in twenty-two hours[*]?

Or how will it account for a storm of hail[†] which fell in Orkney on the 24th of July, 1818, in the afternoon, nine inches deep in less than nine minutes? —ESPY, *Franklin Journal, August*, 1836.

Recent Meeting of German Naturalists, &c.

THE annual meeting of the naturalists and medical men of Germany, commenced at Jena, in the Duchy of Saxe-Weimar, on the 20th ult., and terminated on the 26th, Dr. Kieser, President. The number of members was large. From countries foreign to Germany, there were more from Great Britain than any other. France did not appear to have had one. Among the British visiters, were Professors Daubeny (Oxford,) (distinguished so recently for the effective discharge of his duties as one of the secretaries of the British Association at Bristol,)—Graham (Glasgow,) and—Kane (Dublin.) The Grand Duke of Saxe-Weimar and his court were present, and paid the most marked respect to the objects and members of the Association. The Duke of Saxe-Altenburg founded a premium in Natural Philosophy, for the students of the University of Jena, as a token of the interest he felt in the Association. The meeting for 1837 is to be held at Prague. It was stated that the number of members at Bonn, in 1835, was about five hundred.

Freiburg Suspension Bridge.

WHEN the details of the construction of this extraordinary attempt, to surpass, in span*, every other bridge which had preceded it, were examined in England, considerable doubts were entertained, as to the soundness of the principles which had directed some parts of the arrangements, particularly in that where the engineer had made an angle in the direction of the land-chains, and thus deposited a part of the pull upon the rock, &c., where the angle takes place, instead of carrying the whole pull along one continued line, to the point of attachment at the extremities of the chains, as in Menai-Strait Bridge.

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[*] *Edinburgh Transactions*, 1823.

[†] POUILLET Elemens de *Physique*, II. 758.

From this, or from other, cause have arisen, we regret to say, some appearances of insecurity, and early in this year the authorities of Freiburg temporarily prohibited the transport of heavy loads along it. We shall rejoice to hear that this injunction is removed.

Wanton Destruction annually of a Million Chaldrons of Coal.

As there is no reproduction of Coal in this country, since no natural causes are now in operation to form other beds of it; whilst, owing to the regular increase of our population, and the new purposes to which the steam-engine is continually applied, its consumption is advancing at a rapidly accelerating rate; it is of most portentous interest to a nation, that has so large a portion of its inhabitants dependent for existence on machinery, kept in action only by the use of coal, to economize this precious fuel. I cannot, therefore, conclude this interesting subject without making some remarks upon a practice which can only be viewed in the light of a national calamity, demanding the attention of the legislature.

We have, during many years, witnessed the disgraceful and almost incredible fact, that more than a million chaldrons per annum, being nearly one-third part of the best coals produced by the mines near Newcastle, have been condemned to wanton waste, on a fiery heap perpetually blazing near the mouth of almost every coal-pit in that district.

This destruction originated mainly in certain legislative enactments, providing that Coal in London should be sold, and the duty upon it be rated, by *measure*, and not by *weight*. The smaller coal is broken, the greater the space it fills; it became, therefore, the interest of every dealer in Coal, to buy it of as large a size, and to sell it of as small a size as he was able. This compelled the proprietors of the Coal-mines to send the large Coal only to market, and to consign the small Coal to destruction.

In the year 1830, the attention of parliament was called to these evils; and pursuant to the Report of a Committee, the duty on Coal was repealed, and Coal directed to be sold by *weight* instead of *measure*. The effect of this change has been, that a considerable quantity of Coal is now shipped for the London market, in the state in which it comes from the pit; that after landing the cargo, the small coal is separated by screening from the rest, and answers as fuel for various ordinary purposes, as well as much of the Coal which was sold in London before the alteration of the law.

The destruction of Coals on the fiery heaps near Newcastle, although diminished, still goes on, however, to a frightful extent, that ought not to be permitted; since the inevitable consequence of this practice, if allowed to continue, must be, in no long space of time, to consume all the beds nearest to the surface, and readiest of access to the coast; and thus enhance the price of Coal in those parts of England which depend upon the Coal-field of Newcastle for their supply; and finally to exhaust this Coal-field, at a period, nearer by at least one-third, than that to which it would last, if wisely economized. (See Report of the Select Committee of the House of Commons, on the state of the Coal Trade, 1830, page 242, and Bakewell's Introduction to Geology, 1833, pages 183 and 543.

We are fully aware of the impolicy of needless legislative interference; but a broad line has ben drawn by nature between commodities annually or periodically reproduced by the soil on its surface, and that subterranean treasure, and sustaining foundation of Industry, which is laid by Nature in strata of mineral Coal, whose amount is limited, and which, when once exhausted, is gone for ever. As the Law most justly interferes to prevent the wanton destruction of life and property, it should seem also to be its duty to prevent all needless waste of mineral fuel; since the exhaustion of this fuel would irrecoverably paralyze the industry of millions. The tenant of the soil may neglect, or cultivate his lands, and dispose of his produce, as caprice or interest may dictate; the surface of his fields is not consumed, but remains susceptible of tillage by his successor; had he the physical power to annihilate the Land, and thereby inflict an irremediable injury upon posterity, the legislature would justly interfere to prevent such destruction of the future resources of the nation.

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* The interval between the suspending piers of the Freiburg Bridge is about 870 English feet, and its clear span about 806 feet. The *Menai-Strait Bridge* (same as *its span) is 522 feet.*

This highly-favoured country has been enriched with mineral treasures in her strata of Coal, incomparably more precious than mines of silver or of gold. From these sustaining sources of industry and wealth let us help ourselves abundantly, and liberally enjoy these precious gifts of the Creator; but let us not abuse them, or by wilful neglect and wanton waste, destroy the foundations of the industry of future generations.

Might not an easy remedy for this evil be found in a legislative enactment, that all Coals from the ports of Northumberland and Durham, should be shipped in the state in which they come from the pit, and forbidding by high penalties the screening of any sea-borne Coals before they leave the port at which they are embarked. A law of this kind would at once terminate that ruinous competition among the coal-owners which has urged them to vie with each other in the wasteful destruction of small Coal, in order to increase the profits of the coal-merchants, and gratify the preference for large Coals on the part of rich consumers; and would also afford the public a supply of Coals of every price and quality, which the use of the screen would enable him to accommodate to the demands of the various classes of the community.

A further consideration of national policy should prompt us to consider how far the duty of supporting commercial interests, and of husbanding the resources of posterity should permit us to allow any extensive exportation of Coal from a densely-peopled manufacturing country like our own; a large proportion of whose present wealth is founded on machinery, which can be kept in action only by the produce of our *native* Coal-mines, and whose prosperity can never survive the period of their exhaustion. —BUCKLAND'S *Bridgewater Treatise.*

Clerical Error in the American Patents-Law.

IN all the copies of the American Patents-Law passed in July last, which have reached this country, there is a discrepancy as to the cost of the patent. Sect. 9 states that *thirty* dollars must be paid on an application for a patent; sect. 18 states that *forty* dollars must be paid on an application for an extension of the term of a patent, "*as in the case of an original application for a patent.*" We find it the same in the copy of the law printed in the Franklin Journal, published at Philadelphia in September. This has led to an error in Art. 2 (*a*) and 5 of the Tariff of Fees published p. 252 of this Volume. Upon inquiry, we are assured that to be correct they should stand thus:—

		Dolls.	£ s. d.
2.	Application for Patent,— (*a*) If by a Citizen, or by an Alien-resident of a year, having made oath of an intention to become Citizen ..	30...	6 15 0
5.	Extension of Patent-Term ...	30...	6 15 0

Patent-Law Grievance. No. VIII.

THE penalties inflicted on the inventive genius of Britain during the present year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £36,000!

N.B. This sum has been paid in *ready money*, on taking the first steps, and as many of the inventors are poor men, (operatives,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

NEW PATENTS. 1836.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

SEPTEMBER *cont.*

219. **MOSES POOLE**, Lincoln's Inn, *Middx.*, Gent.; for improvements in anchors and in friction-rollers, to facilitate the lowering and raising anchors, and for other purposes. Sept. 15.—March 15. *For. Comm.*

220. **WILLIAM PRINGLE GREEN**, Falmouth, *Cornw.*, Lieut. R.N.; for improvements on capstans applicable to ships and other purposes, and for contrivances to reduce manual labour at capstans used at mines. Sept. 28.—March 28.

221. **JOHN ISAAC HAWKINS**, Chase Cottage, Hampstead-Road, *Middx.*, Civil-engineer; for an improvement in the blowing-pipe of blast-furnaces and forges. Sept. 28. —March 28. *For. Comm.*

222. **GEORGE CRANE**, Yuiseedywyn Ironworks, Swansea, Iron Master; for an improvement in the manufacture of Iron. Sept. 28.—March 28.

223. **WILLIAM NEALE CLAY**, West Bromwich, *Staff.*, Manufacturing Chemist; for improvements in the manufacture of sulphate of soda. Sept. 28.—March 28.

224. **RICHARD PEARSON**, St. Giles, *Oxford*, Organist; for improvements in drags, or apparatus for retarding carriages. Sept. 28.—March 28.

TOTAL, SEPTEMBER...21.

OCTOBER.

225. **JOHN LEDYARD PHILLIPS**, Melksham, *Wilts.*, Cloth Manufacturer; for an improvement in the manufacture of woollen cloths. Oct. 4.—Dec. 4.

226. **JAMES WHITE**, Lambeth, *Surry*, Engineer; for improvements on railways. Oct. 4.—April 4.

227. **CHARLES WILLIAM STONE**, Finchley, *Middx.*, Mechanic; for improvements in harness, for weaving purposes, and in the apparatus for making the same. Oct. 4.—April 4. *For. Comm.*

228. **HENRY HUNTLEY MOHUN**, Walworth, *Surry*, D.M.; for improvements in the manufacturing of fuel. Oct. 4.—April 4.

229. **SAMUEL TONKIN JONES**, Manchester, *Lanc.*, Merchant; for improvements in the tanning of hides and skins. Oct. 6—Apr. 6.

230. **MILES BERRY**, Chancery-Lane, *Middx.*, Mechanical Draftsman; for improvements in machinery, or apparatus for manufacturing metal screws. Oct. 6. —April 6.

231. **JOHN SHARP**, Dundee, *Forfar*, N.B., Flax Spinner; for machinery for converting ropes into tow, and improvements in machinery for preparing hemp or flax for spinning; part of which improvements are applicable to the preparing of cotton, wool, and silk, for spinning. Oct. 8.— April 8.

232. **HENRY SCOTT**, Jun., and **ROBERT STEPHEN OLIVER**, Hatters, *Edinburgh*, for improvements in the manufacture of hats, caps, and bonnets, Oct. 13.—Apr. 13. *For. Comm.*

233. **FREDERICK GATHNER**, Birmingham, *Warw.*, Brass-founder; for improvements applicable to the drawing or winding up of window and other roller-blinds, or maps. Oct. 13.—April 13.

234. **JOHN HEMMING**, Edward-St., Portman-Sqr., *Middx.*, Gent.; for improvements in the manufacture of white-lead. Oct. 13.—April 13.

235. **THOMAS LUTWYCHE**, Liverpool, *Lanc.*, Manufacturing Chemist; for improvements in the construction of apparatus used in the decomposition of common salt, and in the method of working or using the same. Oct. 13.—April 13.

236. **JOHN RUTHVEN**, *Edinburgh*, for improvements in the formation of rails or rods for making railways, and in the method of fixing or joining them. Oct. 13. —April 13.

237. **CHARLES PIERRE DEVAUX**, Fenchurch-st., *Lond.*, Merchant; for an improved apparatus for preventing the explosion of boilers or generators of steam. Oct. 13.—April 13. *For. Comm.*

238. **JOHN JOSEPH CHARLES SHERIDAN**, Peckham, *Surry*, Chemist; for improvements in the several processes of saccharine, vinous, and acetous fermentation. Oct. 20.—April 20.

239. **WILLIAM BRIDGES ADAMS**, Brecknock-Crescent, Camden-Town, *Middx.*, Coach-Maker; for certain improvements in wheel-carriages. Oct. 20.—April 20.

240. **CHRISTOPHER NICKELS**, Guildford-st., Lambeth, *Surry*, Manufacturer of Caoutchouc, for improvements in preparing and manufacturing caoutchouc, applicable to various useful purposes. Oct. 24.— April 24. For. Comm. in part.

METEOROLOGICAL JOURNAL FOR SEPTEMBER, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer		Daily Temp	Solar Var.	Rad.	Clouds.		Clouds		Direction of wind		Lunation.	WEATHER, &c.
						Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Thurs.	1	29·906	68°	30·000	69°	48°2	68°3	58°2	20°1	47°	5	1	3	2	S.W.	W.		A squall at 10 A.M.; day fine and clear, with wind.
Friday,	2	29·902	66	29·846	68	50·5	61·8	56·1	11·3	48	8	8	2·	3·2	S.S.W.	W.		Heavy showers; *cumuli* and *nimbus*; fine evening.
Satur.	3	30·054	63	29·948	65	43·0	66·5	54·7	23·5	41	5	5	1	1	S.W.	SS.W.E.	☾	Flying clouds; eve. *Cirrus*, and indications of rain.
SUN.	4	29·514	63	29·542	66	50·3	70·8	60·6	20·5	48	5	6	3	2	S.S.W.	S.S.W.		Much rain before sun-rise; windy *scud*.
Mon.	5	29·670	65	29·748	66	52·0	63·1	57·6	11·1	51	3	2	3	2	W.	S.W.		Fine; *cumulus*; night, squalls of wind and rain.
Tues.	6	29·489	63	29·418	64	48·5	62·9	55·7	14·4	48	8	5	3	2	S.S.W.	S.W.		Heavy rain in showers; wind.
Wed.	7	29·751	62	29·803	63	48·2	60·0	54·3	11·8	47	5	7	1	1	N.	N.		Rain at 7 A.M.; large dense *cumuli*; cold hard sky.
Thurs.	8	29·902	62	29·850	63	48·5	63·4	56·0	14·9	47	4	5	1	1	W.	S.W.		*Cumulus*, *Cum.-Stratus* and *nimbi*; little rain fell.
Friday,	9	29·856	62	29·914	63	46·2	65·0	55·6	18·8	45	8	9	1	1	W.	N.W.		Overcast; rainy afternoon; evening starlight.
Satur.	10	29·900	62	29·948	62	42·1	56·3	49·2	14·2	42	8	6	2	2	W.	N.W.		Rain early A.M.; *nimbi*, with showers; cold rain.
SUN.	11	30·132	58	30·100	60	38·6	58·0	48·3	19·4	38	3	8	1·3	2	N.W.	N.W.	●	Sharp air; showers of rain P.M.; cold stormy wind
Mon.	12	30·104	59	30·141	60	48·5	56·8	52·6	8·3	48	9	10	2·3	2	N.b E.	N.b E.		Stormy, with showers of rain. [at night.
Tues.	13	30·169	59	30·160	60	50·8	57·6	54·2	6·8	49	7	8	3	2	N.b E.	N.		Blustering; strong showers; *scud*.
Wed.	14	30·234	60	30·232	61	47·2	58·0	52·6	10·8	46	9	9	2	2	N.N.E.	N.E.		Cloudy.
Thurs.	15	30·275	60	30·250	62	46·5	60·5	53·5	14·0	45	3	9	1	1	E.N.E.	N.E.		Fine morning; cloudy; *cumuli* and *cum-stratus*.
Friday,	16	30·200	61	30·174	61	50·0	59·1	54·6	9·1	48	10	6	2	1	N.E.	N.E.		Cloudy; rainy; afternoon clouds broken; fine even.
Satur.	17	30·196	61	30·151	62	45·8	60·7	53·3	14·9	44	5	5	1	2	N.E.	N.E.	☽	*Nimbi*, with showers.
SUN.	18	30·114	61	30·105	62	48·2	58·0	53·1	9·8	46	8	10	2	2	N.b E.	N.b E.		Overcast throughout. [dark and windy.
Mon.	19	30·097	61	30·056	61	48·4	56·9	52·6	8·5	47	6	10	1	1	N.	N.W.		Much cloud; *cumulus*, *cum.-stratus*; cold P.M.; very
Tues.	20	30·102	60	30·077	61	45·6	58·6	52·1	13·0	44	4	7	1	1	N.W.	N.W.		A light shower at 10 A.M.; fine at noon; P.M. overc.
Wed.	21	30·253	59	30·277	57	36·2	51·9	44·1	15·7	35	8	8	1	1	N.W.	N.N.W.		Much cloud; *cirro-cum.* and *cum.-stratus*.
Thurs.	22	30·396	55	30·331	57	33·0	57·8	45·4	24·8	31	7	8	0	1	S.	S.S.W.		Clear after midnight, with a hoar-frost; cloudy.
Friday,	23	30·092	59	30·006	61	48·5	64·0	56·2	15·5	48	9	10	3	3	S.W.	S.W.		Wind and rain; *cirro-stratus* and *scud*; fine evening.
Satur.	24	30·258	63	30·218	65	53·5	66·0	59·8	12·5	50	3	1	2	3·2	W.	W.S.W.	○	Very fine; loose *cumuli*; cloudy at 6 P.M.; clear.
SUN.	25	30·302	65	30.300	66	58·5	68·2	60·6	15·2	50	7	6	2	2	W.S.W.	W.S.W.		Cloudy; *scud*; mild; very fine warm weather.
Mon.	26	30·262	66	30·160	68	57·0	71·4	64·2	14·4	54	9	3	2	1	S.W.	S.		*Stratus* into *scud* till noon; *cirro-cum.* and sultry.
Tues.	27	29·954	67	29·951	67	58·4	65·1	61·7	6·7	55	10	10	2	2	S.W.	S.W.		Clouds very thick; generally overcast.
Wed.	28	29·806	66	29·745	66	49·8	60·4	55·1	10·6	47	6	6	2	3	S.S.W.	W.S.W.		*Cumuli* into *nimbi* with heavy showers; thunder n.
Thurs.	29	29·400	64	29·450	65	50·2	60·0	55·1	9·8	48	10	10	0	3·2	N.	S.W.		Rain early; gloomy; rainy P.M.; high wind 5 to 6.
Friday	30	29·524	61	29·600	62	45·8	53·2	49·5	7·4	43	6	7	3	3·1	W.	W.b N.		*Nimbi*, with squalls; very cold clear even.; Aurora.
Mean		29·993	62	29·983	63	47·75	61·34	54·56	13·59									

Bar. Max. 30·396 in. on the 22nd. || Ther. Max. 71·4° on the 26th. || Lowest point of Rad. 31°, on the 22nd.
Bar. Min. 29·395 in. 29th. || Ther. Min. 33·0° 22nd. || Rain fallen 3·375 in.

DR. BUCKLAND'S BRIDGEWATER TREATISE.

Geology and Mineralogy considered with reference to Natural Theology.
BY WILLIAM BUCKLAND, D.D. &c. 2 vols. 8vo.

THE BRIDGEWATER TREATISES may be regarded as marking an epoch in our literature: the agreeable and attractive manner in which the most exalted subject which can occupy the mind of man—the knowledge of the Creator and his attributes—has been inculcated in these works, having been productive of as much pleasure combined with instruction as perhaps any equal number of volumes ever published. If, therefore, Dr. Buckland's work had only constituted the concluding one of the series it would have excited great interest, but other circumstances have concurred to attach peculiar importance to his contribution, and these circumstances must be borne, in mind to enable us to appreciate the difficulties which the author had to contend with, and the degree of success with which he may be considered to have overcome them.

The publication of the *Reliquiæ Diluvianæ*[*] has caused Dr. Buckland to be looked upon as the champion of that party which views with jealousy and suspicion those discoveries made by geologists as to the past history of our globe, which appear at variance with the Mosaic account of the Creation. Most of our readers may recollect the triumph with which that work was received as a refutation of these deductions. Scientific men, who had simply, in good faith, promulgated the results of their investigations had been accused of infidelity, and, in the blind zeal to expose them, the charity which ought to have attributed their supposed errors to defective judgment, rather than to sinister intentions, and the common sense which should have suggested that it was another, and equally authentic revelation of Divine power[†], which they were attempting to interpret, were alike forgotten.

We fear it was as much this party feeling as Dr. Buckland's acknowledged qualifications for the task, that caused him to be selected to write the treatise on Geology for the Bridgewater series; and the result of his labours was, naturally, expected with anxiety by two classes of readers. The religious alarmists hoped that they would produce an explanation of the facts recently ascertained as to the early state of our planet, which would be in accordance with their views; for the scrupulous adherence to the purest spirit of inductive philosophy, which has characterized the prosecution of this branch of science, during the last twenty years, has given an authority to the deductions promulgated by its cultivators, that cannot be shaken by mere declamatory arguments.

On the other hand, those who had received these deductions as physical truths, without reference to their bearings on a subject with which they had properly no connexion, were curious to ascertain how far Dr. Buckland would feel himself obliged to modify his opinions, in consequence of the facts brought to light since the publication of his former work. They knew that even in the *Reliquiæ Diluvianæ*, though the truth had been told, it was not the *whole truth;* enough had been stated to furnish arguments to those who, unable or unwilling to examine for themselves, were desirous of decrying opinions repugnant to their feelings, but the suppressed evidence was as fatal to these arguments as that brought forward appeared favourable to them, and the time was now arrived, when that evidence was too generally known and admitted by competent judges, not to be openly allowed by all*.

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[*] *Reliquiæ Diluvianæ*, or Observations on the Organic Remains contained in Caves, Fissures, *and Diluvial Gravel:* and on *other Geological Phenomena*, attesting the Action of an Universal Deluge.—4to., 1823.

[†] The "book of God's works" as Lord Bacon styles Nature.

It will be, therefore, acknowledged that Dr. Buckland had a difficult and a delicate task to execute in his present work. His character as a scientific man required from him a full exposition of those geological facts which had been authenticated, and of the conclusions which followed from them, whether they were in accordance or not, with the doctrines he had previously advocated; while the unfortunate association between the truths of a physical science and those of a moral revelation, which he had so essentially assisted in establishing, compelled him again to enter into a discussion, alike injurious to true religion and to sound knowledge. Having considered the subject in its theological bearing in another place, we shall no further allude to this discussion than to point out how Dr. Buckland in his present work has erred, in our opinion, by again mixing up two such incongruous matters. The object of the Bridgewater treatises, according to their founder's intention, was *to illustrate the power, wisdom, and goodness of God, manifested in the creation;* in short, both according to the words, as well as the spirit of the bequest, they were intended to form a body of *Natural Theology*, in which the arguments adduced by Paley and others should be elaborated, extended, and corrected, according to the improved state of our knowledge of the material universe. It was by strictly adhering to this plan, and by sedulously avoiding reference to Revelation, that Dr. Buckland's colleagues, with a single exception, succeeded in producing a series of works equally instructive and beneficial; this path was open to that gentleman, and the example of Mr. Kirby should have warned him of the mischief that must accrue from deviating from it; but the author of the *Reliquiæ Diluvianæ*, fettered by the load of his previous reputation, appears to have been unwilling entirely to surrender a position by the defence of which it was acquired; and has consequently missed the opportunity of establishing a more permanent and more desirable celebrity, by becoming the author of a work which should be equally characterized by sound knowledge, philosophical reasoning, and by its freedom from polemical disquisitions.

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* We must refer, generally, on this subject, to the article on Geology in our last number; nevertheless we will here briefly recapitulate the evidence by which the assertion in the text is supported.

Dr. Buckland's object, in his former work, was to show, "that the general dispersion of gravel and loam over hills and elevated plains as well as valleys, was the effect of an universal and transient deluge;" that the general form of valleys, their mutual connexion and ramifications, could only be accounted for on this supposition; that the evidence derived from the races of animals destroyed at the period, and the tradition throughout all nations of such a cataclysm, were collateral proofs of it; and that the event had occurred about 6000 years ago. *Therefore* these effects were the result of the Noachian deluge.

"In the full confidence that these difficulties will at length be removed, by the further extension of physical observations, we may for the present rest satisfied with the argument that numberless phenomena have been already ascertained, which, without the admission of an universal deluge, it seems not easy, nay, utterly impossible to explain." (*Reliq. Dil.*) "And by affording the strongest evidence of an universal deluge leads us to hope that it will no longer be asserted, as it has been by high authority, that geology supplies no proofs of an event, in the reality of which the *truth of the* Mosaic records is so materially *involved.*" (Dedication to the Bishop of *Durham.*)

Our readers are aware that Dr. Buckland has acknowledged that subsequent discoveries have invalidated the justness of his conclusions deduced from organic remains, but he has not, that we know of, exposed the other weak points in his arguments, which it did not require any additional facts to make evident.

Though there was physical evidence of the violent effects of water over the *whole known* surface of the earth, as far as it had been examined, yet there was, not only, none that it had acted *simultaneously*, even at any two adjoining localities, but the very circumstance that the traces were those of *violent* currents, excavating valleys, depositing gravel, boulders, &c., on hill-sides and tops, was a direct proof that the water had *not* acted universally and simultaneously. If we suppose a mountain lake to burst its barrier, or a subterranean movement to elevate a large extent of the bed of the ocean, the hydraulic action of the water so displaced might produce such effects as those mentioned. But no laws with which we are acquainted could cause currents, adequate to that purpose, in an ocean gradually elevated above the summits of the highest mountains; and Dr. Buckland must have been aware that, except by the immediate will of a Divine Power, suspending at his pleasure the otherwise immutable laws of nature, no universal deluge ever could have occurred on the earth. The attempt, therefore, to establish the truth of a miracle by human reasoning was in this, as in every other case, as unphilosophical as it was fruitless.

Ever since the futility of all attempts to construct a "theory of the earth," in the present state of our knowledge, was distinctly recognised, it has been the proud boast of the cultivators of geology that they sedulously avoided all speculative hypotheses, and confined themselves to the accumulation of facts; it is the strict adherence, by the leading geologists of our times, to this precept, that has raised the science to its present pre-eminence, and has made the study of it attractive to well-disciplined minds.

The first prominent defect that strikes us in the outset in Dr. Buckland's book, is occasioned by the disregard of this rule; having commenced with a disquisition on the verbal interpretation of the Mosaic cosmogony, and having endeavoured to prove that there is no discrepancy between that record of creation and the results of our observations on the successive conditions of the earth, Dr. Buckland could hardly avoid entering into an account of the first emergence of the globe from a chaotic state, and by so doing, he has, in some measure, blighted the ripening fruits of the more philosophical mode of proceeding, of his cotemporaries.

But failure accompanies this as well as every other attempt to substantiate revealed truth by the collateral aid of finite intelligence. Dr. Buckland's cosmogomy has not the slightest connexion with that of Moses, and is obscure and unintelligible; although the deductions of physical astronomy render it highly probable, that our globe may have, at one time, been in a semi-fluid state, yet no knowledge we at present possess can enable us to frame, with anything approaching consistency, an account of its transition from a level, uniform, spheroidal surface, acquired by rotation, to its present irregularity, both in density and level. This subject has been touched on by a cotemporary geologist of the first rank, and we gladly avail ourselves of his words.

"*It is difficult for a speculator* to believe, that Geology may become *a very important branch of* natural science, though it should wholly dis-claim the investigation of problems concerning the creation or concentration of the matter of the globe, or the establishment of the laws of the universe. To know the successive changes which the globe has undergone, and thus to trace a retrospective outline of its successive conditions, is actually attempted by geology; but the very processes employed in this enterprise are founded on the recognition of the existing laws of nature, and altogether exclude the popular notion of a chaos, and the philosophical hypotheses of a solid globe, condensing from an atmospheric expansion."

"Undoubtedly the progress of legitimate geology teaches us that the same laws of nature have operated on this globe under very different circumstances, as to temperature, relation of land and sea, animal and vegetable life, and many other things, and it is become a proper problem for geology to discover these circumstances. In this point of view, the reflections of Leibnitz and the mathematical labours of La Place and the astronomers, become of great value, since they help to fix conspicuous landmarks for the guidance of the surveyors in this large field of science; but let no one delude himself with the notion of discovering by geological processes the emergence of the harmoniously-adjusted terraqueous globe from a former state of chaos. It is certainly not a philosophical, and surely cannot be thought a religious notion, that man shall ever discover, among the works of God, the traces of a period when his divine attributes were first awakened to rescue his creation from anarchy. Geology takes for granted the existence and collection of the matter of the globe, with its supernatant ocean and its enveloping atmosphere. Except in the degree of influence which circumstances permit them to exert, it takes for granted the uniformity of action of all material causes. The investigation of miracles never can be admitted into natural science." (Professor Phillips, *Art.* Geology, *Encyclop. Metropol.*)

Accordingly we regard Dr. Buckland's account of the original state of the globe, and of the solidification of its surface by radiation, oxidation, &c., brief and general as it is, as positively mischievous in an elementary work, intended for instruction to persons not conversant with philosophical reasoning, because it tends to give them erroneous notions as to the proper objects and limits of the science.

Dr. Buckland has obviously aimed at making his work popularly attractive, but he has done so at the expense of making it beneficial. No one totally ignorant of geology could acquire any connected outline of information on the subject, from his volumes alone. We think he judged rightly, in omitting purely mineralogical details, and in referring his readers to other works for that information; but, by altogether passing over the investigation of the modes in which existing agencies are ceaselessly engaged in removing and renewing the inequalities of the earth's surface, in transporting to the ocean, by means of running water, the materials for new strata, analogous in their characters to those which now constitute the stratified crust of the globe, and by avoiding discussion on the probable action of subterranean forces in modifying these and former deposits,—he has voluntarily renounced a fertile source of interest and instruction, no wise inferior to that which he has explored. It is true, that accounts of the discoveries of pre-existing organized beings, differing in form from those now inhabiting the globe, and of the series of inductions by which anatomists are enabled to read the history of an extinct species in a small portion of its relics, may appear more captivating than dry details of mere chemical and mechanical actions ceaselessly modifying the surface of our planet. But the two classes of phenomena are too intimately connected with its history, to allow of the one being correctly treated independently of the other; and an author of Dr. Buckland's rank and authority should rather have directed his readers in the right path to knowledge, than have humoured an idle taste for what was especially amusing. The existence of this taste being strongest in least cultivated minds, it becomes the imperative duty of a teacher to control it by strict mental discipline; unfortunately the necessity for making the acquisition of knowledge pleasing, induces a constant violation of this rule; and popular works on scientific subjects are too often calculated to convey erroneous notions on the precise limits between what is really ascertained as fact, and what is the result of speculative induction. Works on geology are peculiarly liable to this defect, from the nature and variety of the subjects they treat of; and yet no science has really less need for such extrinsic aid; the number and singularity of the facts which it develops are quite sufficient to invite a study of them by any one capable of appreciating the beauty of an extensive and connected chain of evidence, diligently accumulated and cautiously examined.

If it be alleged that the primary purport of the publication was to teach natural theology, and not the history of the earth, and therefore, that portion of the latter was most dwelt on which furnished the greatest number of arguments bearing on the principal object*,—we reply, that the value of any argument adduced from a physical science must depend on the reader's conviction of the authenticity of the facts on which it is based, and of the soundness and consecutiveness of the deductions made from them; to enable a reader, consequently, to judge for himself, he ought to have a complete general outline at least of the science laid before him; this plan having been successfully adopted by Messrs. Whewell, Kidd, Drs. Roget and Prout, Dr. Buckland might have followed such examples without derogation, and with more probability of making his work efficient. This mode of proceeding is the more necessary in the present case, because most persons are capable of following the train of reasoning by which the principal conclusions in geology are arrived at, supposing the facts to be authentic, and these the general reader must take on the authority of his author, whatever the subject may be he is studying.

Nor are the evidences of design and adaptation in the details of the organic creation, more convincing proofs of the existence of an eternal, intelligent, and omnipotent First Cause, than is the constancy of the laws by which inorganic matter is governed. The deductions of geology, as we have seen, are based on the assumption of this constancy, and astronomy has established the fact regarding the all-pervading laws of gravitation. If, then, the science we are considering can be shown to afford analogous presumptive proofs regarding others, the argument would be interesting and important enough to deserve more extended notice than Dr. Buckland has bestowed on it. One of the most striking, because novel, facts mentioned in his work is the discovery of the eyes of extinct crustacea, which by their resemblance to those organs in living races, indicate the identity of the light of the primæval world with that element which now pervades space. Geology offers numberless other conclusions of an analagous kind with respect to inorganic creation: fossil plants, by their similarity in structure and developement to existing species, may be fairly concluded to have been nourished by water and air of the same chemical, as well as mechanical, properties as that which fills our seas, rivers, and lakes, and descends from the clouds in rain and dew. The arrangement of the component ingredients of conglomerates and breccias reciprocally establish the permanence of their specific gravity, and of the hydrostatic pressure of the water which floated them to their present sites; and as the gravitation has been shown to be constant, the other laws, chemical and mechanical, may be inferred to be so likewise. These presumptive proofs being established, the nature of the evidence on which geological deductions rest becomes of more weight.

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* When a variety of examples only concur to the establishment of the accuracy of one line of argument, it is sufficient to develop that argument *once, in all its generality, and to leave the individual instances of its application to speak for themselves.* Dr. Buckland has repeated the conclusion, that design is to be inferred from the evidence of adaptation to an end, so frequently, and so nearly in the sam terms, as to be irksome, if not positive ludicrous.

Having fulfilled the least agreeable part of our duty, we may now indulge in the pleasure of pointing out a few of the many beautiful illustrations and philosophical deductions, which the reader will find in Dr. Buckland's work, expressed with that force and grace which make both his writings and lectures so popular. We would especially call attention to the following passage, as inculcating a principle in which man is but too much deficient—humility, and which ought to have double force as coming from a philosopher and a divine.

"I would in this, as in all other cases, be unwilling to press the theory of relation to the human race so far, as to contend that all the great geological phenomena we have been considering were conducted *solely* and *exclusively* with a view to the benefit of man. We may rather count the advantages he derives from them as incidental and residuary consequences; which, although they may not have formed the exclusive object of creation, were all foreseen and comprehended in the plans of the great Architect of the globe, which in His appointed time was destined to become the scene of human habitation.

"With respect to the animal kingdom, we acknowledge with gratitude that among the higher classes there is a certain number of living species which are indispensable to the supply of human food and raiment, and to the aid of civilized man in his various labours and occupations; and that these are endowed with dispositions and faculties which adapt them in a peculiar degree for domestication: but their number bears an extremely small proportion to the total amount of existing species; and with regard to the lower classes of animals, there are but very few, among their almost countless multitudes, that minister either to the wants or luxuries of the human race. Even could it be proved, that all existing species are *serviceable to* man, no such inference could be drawn with respect to those *numerous* extinct animals which geology shows to have ceased to live *long before* our race appeared upon the earth. It is surely more consistent with sound philosophy, and with all the information that is vouchsafed to us respecting the attributes of the Deity, to consider each animal as having been created, first for its own sake, to receive its portion of that enjoyment which the Universal Parent is pleased to impart to each creature that has life; and secondly, to bear its share in the maintenance of the general system of co-ordinate relations, whereby all families of living beings are reciprocally subservient to the use and benefit of one another. Under this head only can we include their relations to man, forming, as he does, but a small, although it be the most noble and exalted part, of that vast system of universal life with which it hath pleased the Creator to animate the surface of the globe." (Vol. i. p. 101.)

One very striking and satisfactory result of the investigations of extinct races of animals, is the discovery of those links in the great chain of organized beings which were wanting to its continuity. We possessed long detached segments of this chain, but we searched in vain among living genera for these connecting portions; geology is now come to the aid of natural history, and from the numerous contributions it has already furnished, we may hope it will in time enable us to complete what is still deficient, and establish the truth of the dictum, *Natura non facit saltus.*

The order *Pachydermata*, as our readers are aware, was particularly deficient in genera to constitute intermediate links between such remotely allied animals as the horse, hog, rhinoceros, hippopotamus, tapir, and elephant; the numerous extinct genera of this order, already discovered, fill up these hiatuses.

"This numerical preponderance of pachydermata, among the earliest fossil mammalia, beyond the proportion they bear among existing quadrupeds, is a remarkable fact, much insisted on by Cuvier; because it supplies, from the relics of a former world, many intermediate forms which do not occur in the present distribution of that important order. As the living genera of Pachydermata are more widely separated from one another than those of any other order of mammalia, it is important to fill these vacant intervals with the fossil genera of a former state of the earth; thus supplying links that appeared deficient in the grand continuous chain which connects all past and present forms of organic life, as parts of one great system of creation." (Vol. i. p. 88.)

Besides filling up the gaps in the order, some of the fossil genera of Pachydermata form points of connexion between that and the orders Ruminantia and Edentata, and one, the *Dinotherium*, cannot properly be assigned to any one of these orders exclusively, on account of the singular anomalous formation of its *lower* jaw, terminating in two tusks projecting downwards, like those from the *upper* jaw of a walrus.

Most of those species which belong to extinct genera agree in some points with several widely dissimilar living species, which are by this means brought into closer union,—and not only orders, but even classes, are placed in unexpected affinity by means of these inhabitants of a primæval world.

The *Saurian Reptiles* and *Fishes* are connected by the gigantic *Ichthyosaurians;* but a still more marvellous example of the union of characters of *remote classes* is presented by that mystery, the Pterodactyli *which were intermediate to* the Saurians and the Cheiroptera.

The wonderful and imposing fact of the successive creation and extinction of species is brought before the mind with peculiar force, when we absolutely find the relics of an extinct order still lingering, as it were, on the globe, and perhaps destined at no distant period to die out like their ancestral *family*. We may fancy the Polypteri of the African, and the Lepidostei of the American rivers, viewing with indifference a creation in which they feel themselves *out of their places*, and recalling those periods of Ichthy*al* grandeur, when the aristocratic supremacy of Megalichthys was acknowledged throughout the deep, and the lordly Sauroids were legitimate monarchs by the strength of their teeth. We doubt not the Bichir of the Nile often views the crocodile with envy, and repines, *like other creatures*, at the partiality of Providence, which has shown such favour to a *modern* branch of their noble stock, while the true representative is struggling with adversity, and only holds the precarious tenure of its existence by the compassionate forbearance of such plebeian upstarts.

It would be a curious and interesting object of inquiry to find whether, in the present state of our knowledge of the relations between organic life and the inanimate world, we possess any data for venturing at a guess what existing species among the higher orders will next become extinct, in obedience to this law of succession, which seems to form a part of the code of Creation; and to trace the probable effect of human agency in modifying the natural progress of this event,—whether there exists a generic power of continuance in certain races which will effectually counteract the unremitting persecution and war of extermination carried on against them, in consequence either of the real wants, the cupidity, or the cruelty of mankind. It is most probable, however, that we have but little influence beyond keeping down the numbers of certain genera or races, and that it is only because the accurate adjustment between the organic functions and the external world is deranged, that a race of animals becomes extinct. If it be true that in the Dodo we have an instance of a genus becoming extinct since the creation of the human race, how much must we regret that we possess such meagre information of the circumstances.

The incorrectness of the conclusion, which had been hastily adopted from insufficient geological researches, that there had been a progressive advance from the simpler to the more complicated forms of organic structure, has been established by different and conclusive arguments, suggested by more accurate knowledge. The avowal, that we know not what class of animals are to be considered as a standard for estimating relative simplicity or complexity of structure, is extorted from us by Ehrenberg's investigations of Infusoriæ; while the discovery of types of the higher classes, as we term them, in the more ancient strata, prove that at all periods there has existed a proportional variety among cotemporary species, as at present. In the following passage we have laid open to us a new source of error to be guarded against, in our attempts to interpret the state of organic creation in remote eras.

"The values to be attached to *numerical* proportions of fossil plants, *in estimating* the entire condition of the Flora of these early periods, has *been diminished* by the result of a recent interesting experiment made by

Professor Lindley, on the durability of plants immersed in water. Having immersed in a tank of fresh water, during more than two years, one hundred and seventy-seven species of plants, including representatives of all those which are either constantly present in the coal-formations, or universally absent, he found,

"1. That the leaves and bark of most dicotyledonous plants are wholly decomposed in two years, and that of those which do resist it the greater part are Coniferæ and Cycadeæ.

"2. That Monocotyledons are more capable of resisting the action of water, particularly palms and scitamineous plants; but that grasses and sedges perish.

"3. That fungi, mosses, and all the lowest forms of vegetation, disappear.

"4. That ferns have a great power of resisting water, if gathered in a green state, not one of those submitted to the experiment having disappeared; but that their fructification perished.

"Although the results of this experiment, in some degree, invalidate the certainty of our knowledge of the *entire* Flora of each of the consecutive periods of geological history, it does not affect our information as to the number of the enduring plants which have contributed to make up the coal-formation; nor as to the varying proportions, and changes in the species of ferns and other plants, in the successive systems of vegetation that have clothed our globe.

"It may be further noticed, that as both trunks and leaves of Angiospermous dicolyledonous plants have been abundantly preserved in the tertiary formations, there appears to be no reason why, if plants of this tribe had existed during the secondary and transition periods, they should not occasionally have escaped destruction in the sedimentary deposits of these earlier epochs."—P. 481.

Dr. Buckland has not dwelt sufficiently on the various causes which influence the preservation in strata of parts of organic beings; nor on the circumstances that cause a far greater numerical proportion of the remains of certain classes and orders to be found than of others. Without this commentary, the nature and value of the evidence derived from this source cannot be properly appreciated; unless thus forewarned, the first hasty conclusion which would be drawn, is, that the proportion between the numbers of species of which the remains are preserved, was the same as that of the species existing at the period.

But when it is shown that marine animals, generally, are placed in circumstances most favourable to the preservation of their solid parts, and conchiferous and molluscous more so than fish, and these again more than amphibious reptiles,—and that it must require a rare combination of circumstances to allow of the skeletons of birds, or the exuviæ of insects, being preserved from that decomposition to which the most solid parts of all organic beings are subject from exposure to atmospheric influence; the student is made aware of the caution necessary to be used in drawing inferences as to the state of living creation at any former period, from the fossil remains of that time. Perhaps in the strong cases we have cited in illustration, his unaided judgment would not far mislead him; and he would infer from analogy, the existence of a proportional number of insects at all periods of the Creation, to serve as the intermediate agents in the ceaseless routine of conversion of animal into vegetable bodies, destined again to furnish nutriment to the former; but it is necessary he should constantly have these general principles present to his mind, to apply before he draws any conclusions on the subject of the numerical proportion of living beings of the most nearly related orders, since, even among these, habits and modes of life may concur to favour the preservation of the remains of certain genera, while those of others may strongly militate against the preservation of any records of them.

We are here obliged to close our remarks, and we are less reluctant to do so, knowing how much has appeared elsewhere in our own as well as in the pages of cotemporary works; the volumes are, indeed, replete with the most interesting and valuable information on the subject of fossil remains; and our regret that Dr. Buckland did not make the plan of his work more comprehensive, shows how highly we estimate both the extent of his knowledge and his powers of instruction.

We will not conclude our notice without expressing admiration at the taste and judgment shown in the selection and execution of the plates. The wood-cuts, designed and engraved by Mr. Fisher, place him in the first rank of his invaluable profession, and the exquisite skill and talent of Mr. J. D. C. Sowerby as an artist are well known. Dr. Buckland will not take it as a bad compliment, if we say that his second volume alone would entitle him to the gratitude of every naturalist and geologist.

THE PHILOSOPHY OF THE HOUR-GLASS.

In olden time, long ere the art of clock-making was discovered, our ancestors marked the fleeting hours by the flowing of sand in a glass. This contrivance was called the hour-glass, and it is still very generally to be found upon the table of the public lecturer, or the private teacher, in the laboratory of the philosopher, or in the cottage of the peasant. It is a far more accurate measurer of time than is usually imagined, and, therefore, perhaps a short account of the theory of its action may be acceptable to the readers of the *Magazine of Practical Science.*

The investigation was undertaken a few years since by M. H. Bournand: his experiments are exceedingly curious, and merit to be more generally known. A few only of the most remarkable, and easy of performance, are detailed in the following notice.

The first remarkable fact regarding the hour-glass is, that the flow of its enclosed sand is perfectly equable, whatever may be the quantity contained in the glass, at any period of its flowing: or, in other words, that it runs no faster when the upper cone is quite full, than when it is nearly empty. This is contrary to what we might expect, for it would be natural enough to conclude, that when full of sand, the lowest particles would sustain a greater pressure from the incumbent mass, and, therefore, be more swiftly urged through the aperture, than when only a quarter full, and near the close of the hour.

The fact that the flow is equable, at any period, may be proved by a very simple experiment.

Provide a quantity of what is called *silver-sand*, (well known for domestic use,) dry it upon a hot stove-plate, or in an iron ladle over the fire; then sift it through a tolerably fine sieve, carefully removing all lumps of clay or stone, which are frequently found in it. Next, take a tube of any material, length, or diameter, closed at one extremity, and in the bottom make a small aperture, say the eighth of an inch, place the finger over this, or stop it lightly with a small plug, and then fill up the tube with the sifted sand.

Hold the tube *steadily*, or affix it to a wall, or a frame, at any convenient height from a table, and then removing the finger, or plug, permit the sand to flow in any measure, for any given time,—supposing into a common graduated glass measure, for a quarter of a minute. A certain quantity is thus obtained, which must be noted. Now let the tube be only a half or a quarter full of sand, and begin to measure again, for a like time, the same quantity of sand will flow; and even if by means of a ruler or plug, the sand in the tube is violently pressed upon, the flow of the sand from the aperture will not, in the least degree, be accelerated, provided the tube is kept steady, and the experimental comparisons all accurately and fairly made. Now all this admits of a simple and satisfactory explanation. Sand, if allowed to fall quietly upon any surface, will form itself in a conical heap, having an angle of about 30°; this is seen in the lower cone of the hour-glass, or can be shown by letting the sand fall from the aperture of the tube just mentioned. Whenever a load of dry sand is thrown from a cart or barrow, or sifted through a screen, by the builder in making mortar, it forms a like conical heap, having an angle of 30° or 35°. Now, then, observe the application of this fact of every day occurrence; it will show how intimately "things familiar" are connected with practical science.

As sand thus falls at a determinate angle, it is easy to imagine that when poured into the tube, it must fill it with a succession of conical heaps, and that all the *weight* which the bottom of the tube sustains, is only that of the heap which *first* falls upon it, and that the succeeding heaps are thus prevented from exerting any *perpendicular* pressure upon the bottom, but that they only exert a *lateral* pressure against the walls of the tube.

When pressure is applied to the top of the sand, it is only transmitted *laterally*, and that to a very little extent; consequently the lowest heap of sand enjoys its flow uninfluenced by the strata or pressure above it. This is the reason why the hour-glass flows with such regularity; and that any given base sustains no weight of sand but that of the first heap which falls upon it, and is in immediate contact with it, may be proved by taking a tube, about an inch diameter, open at both ends, wetting with the lips the edges of a small piece of tissue-paper, and applying this to one end of the tube, so as to form a bottom held on by a very slight adhesion. Fill this tube carefully with any weight of sand, and the paper will not be forced away, not even if with a round ruler or rod great pressure is applied to the top surface of the sand. All the *weight of sand that the* tissue-paper supports is the little heap which first *falls upon it.*

If the experiment is made upon a larger scale, with tubes three or four inches in diameter, and five or six feet long, it is better to paste the paper round the bottom, because the first heap in such case would be of considerable weight; but if the paper is strong enough to resist *it*, forty or fifty pounds of sand may be poured into the tube, and all lifted from the ground together, without the slightest fear of the paper being forced away.

The experiment admits of another modification. Take the tube open at both ends, and place one of them in contact with the bottom of a small cup floating upon water, then fill up the tube with sand; none of it will run out into the cup: and that there is no perpendicular pressure is evident from the cup still continuing to float; it sustains no weight save that of the first heap,—the hand of the operator sustains the weight of all the rest of the sand. Draw away the tube, the sand then rushes into the cup in obedience to the law of gravitation, and its weight causes the cup to sink.

From such experiments it may be concluded that it must be extremely difficult to *thrust* sand out of a tube, by means of a fitting plug or piston; and this upon trial is found to be the case. Fit a piston to a tube, (exactly like a school-boy's pop-gun,) pour some sand in, and try with the utmost strength of arm to push out the sand. It will be found impossible to effect this; rather than the sand should be propelled, the tube will burst *laterally*.

Directions are often given by naturalists for shooting birds with a a charge of fine sand, so that their plumage may not suffer the damage which is usually occasioned by an ordinary charge of small shot. This proceeding ought to be made with considerable caution, for it must be evident, from the last experiment, that a charge of sand would resist the expansive force of gunpowder, and violently strain the barrel, perhaps burst it; to say nothing of goring, and spoiling its polished interior by the rapid friction of the sand; and, supposing it in very small quantity, and successfully propelled, it is certainly rather a hazardous experiment. To prove how a small column of sand will resist the expansive force of a large charge of gunpowder, it will be sufficient to instance the method adopted by engineers for blasting rocks.

A hole is drilled in the rock, of the requisite depth, at the bottom of which the charge of powder is placed; a long match, or reed, filled with powder, is then put down, and around this sand is merely *poured in*, so as to fill the hole; a train is laid and fired, and presently the explosion takes place, rending away the mass of rock. The loose column of sand is not *blown out* of the hole leaving the rock unshaken, but it keeps its place, until it compels the solid rock to yield unto its singular power.

The discoverer of these facts, relating to the flow of sand in the hour-glass, makes the following observations.

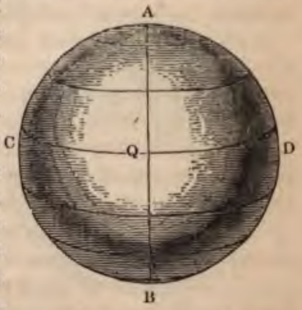
"There is, perhaps, no other natural force on the earth which produces by itself a perfectly uniform movement, and which is not altered either by gravitation, or the friction, or resistance of the air; for the *height* has no influence, *friction* in place of being an obstacle is the *regulating* cause, and the resistance of the air within the column must be so *feeble as to* be altogether insensible as a disturbing force."

A POPULAR COURSE OF ASTRONOMY.

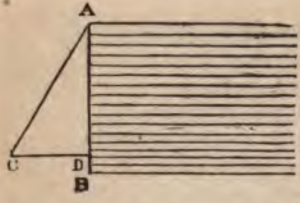
No. VI.

Day and Night.—The Seasons.

There have now been placed before the reader those first truths of astronomy on which the whole fabric of the science may be considered to rest. The infinite distance of the region of the fixed stars, the entire isolation of the earth in space, its spherical form and huge dimensions, its *daily* revolution round one of its own diameters, and its annual revolution round the sun. In this chapter, some of those great phenomena of the visible world which *result* from these will be brought under his consideration. First among these are the alternations of day, of night, and the changes of the seasons. The sun is the source of light and heat; these are facts of which the experience of every day of our lives constitutes the demonstration; they are so plain and palpable that no one was probably ever found to deny them. It is a matter also of daily experience, that a certain class of bodies called opaque, to which class belong by far the greatest number of the bodies around us, have the power of obstructing the light, and also, in a great measure, the heat of the sun; so that whilst the light and heat fall and exert their full influence on one side of them, the opposite is wholly deprived of that influence; the one side is then said to be enlightened and heated, and the other, where there is an entire absence of light and heat, to be in a state of darkness and cold. Now let us suppose a body thus opaque to be turned round, so that what was before the part turned *towards* the sun, may now be that *from it;* that part will be found to have retained none of the light which it received in its first position, so as to be now wholly and absolutely in a state of darkness; but it *will*, on the contrary, have retained a larger portion of its *heat*, so as *not* to be wholly and absolutely in a state of cold. Let us suppose the body a sphere; then will the enlightened portion of it be a hemisphere, that is, one half of the whole, and the division of the light and the dark part of it will be a great circle, A C B D, of the sphere. If the sphere be turned round one of the diameters, A B, of this circle, the positions of its light and dark parts will eventually and gradually be interchanged,—the whole of what was dark before will now be light, and the whole of what was light before, will be dark; and if the revolution of the sphere be continued *uniformly*, that is, always with the same velocity, then each point in it will continue as long on the light side as on the dark side,—as long on the side on which it *receives* heat, as on that on which it does not receive it. The quantity of light and heat which any point receives during the time of its revolution through the enlightened hemisphere is not, however, the same in all the positions which it may be made



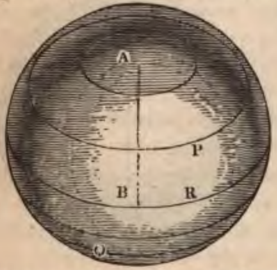
to take up on that hemisphere. Rays, as they are called, of light, and with them rays of heat, come in right lines; and from hence it follows, that any surface presented perpendicularly to them will receive and be acted upon by *more* of these rays than the same surface subjected to their influence *obliquely*. This will at once be rendered evident by a diagram.



Let A B represent a surface presented perpendicularly to the sun's rays, all those represented in the figure as lying between the points A and B will then take effect upon it. Now, let it be turned into the position A C, and let C D be drawn parallel to the direction of the rays; or perpendicular to A B. The rays lying between A and D are manifestly the only ones which now take effect upon the plane, those between B and D being lost. Thus, then, it appears that the same surface receives less light and heat when exposed obliquely than when exposed *directly* to the sun's rays; and the less as its position is more oblique.

Now of the surface of the hemisphere of which we have spoken, only one exceedingly minute portion is exposed perpendicularly to the sun's rays; this portion lies about the point Q, (see the cut on the preceding page,) where a tangent to the sphere is perpendicular to the direction of the rays. Every other portion of the surface receives the rays more or less obliquely, according as it is nearer or more remote from this one particular spot. It is manifest that there will thus arise a very unequal distribution of light and heat. In the revolution of the body that portion which revolves through the point Q, will receive the greatest share; and those portions which are made by the motion of the whole to recede least from the edge of the enlightened hemisphere, will receive the least; thus if the sphere revolve about an axis going through A and B, it is about these points that there will be received in the aggregate the least of light and heat.

Now let us suppose the sphere, instead of revolving round one of the diameters of the circle which divides its dark and its enlightened hemispheres, to revolve round some other axis, as represented in the accompanying figure. The distribution of the light and heat on the surface of the hemisphere will be precisely as before; but by the revolution of the sphere, the different parts of its surface will be made to partake very differently in it. Those, for instance, about the pole A will never, by the revolution of the sphere, be made to pass out of the enlightened hemisphere, whilst those about the opposite pole, B, will never pass out of the darkened hemisphere. Points of the surface situated about P will by the revolution of the sphere be made just to pass *beneath* the *boundary of light* and darkness, whilst those about Q will only just be made *to pass* above it. Thus, in the one place, there will be the least *conceivable portion* of darkness, and in the other the least conceivable light.



Between these extremes, and on the surface extending from P to Q, may be found every conceivable proportion of light and darkness. The points nearer to P, will be kept longer on the light side of the sphere than on the dark one, whilst those nearer to Q, will be kept longer on the dark side than the light. Now whilst any point of the sphere is receiving light, it is receiving heat, and when it is not receiving light, it is cooling or giving out, its heat. There is therefore every possible variety between P and Q, in the proportion of the periods during which heat is received and given out; and from this cause, if it operated alone, there would arise an exceedingly unequal distribution of temperature upon the surface of the globe, resulting ultimately from the fact of the axis, about which it revolves, not lying in the plane of the circle which separates its dark and enlightened portions. It is clear, nevertheless, that this cause of difference of temperature in some degree modifies that which has been before described as dependent upon the greater or less obliquity of the surface to the direction of the incident light and heat. Thus the parts immediately about the pole A, being more obliquely situated than those about the central portions of the sphere R, will on that account receive less heat. But then by the revolution of the sphere, these are never carried into the shadow,—they are therefore *continually* exposed to the action of the heat. Whilst about R each portion of the surface *receives* it only during a portion of its revolution, and throughout the whole of the remainder gives it out.

Thus by this supposed position of the axis, the unequal distribution of temperature arising from different obliquities of the surface is in some measure remedied. And if the position of the axis in reference to the enlightened surface, or of the enlightened surface in reference to the axis, were made to go through a state of continual change, this equalization of temperature might be carried on to almost any conceivable extent.

Now this is precisely the description of change which is going on continually on the surface of the earth, and from which result the seasons of our year.

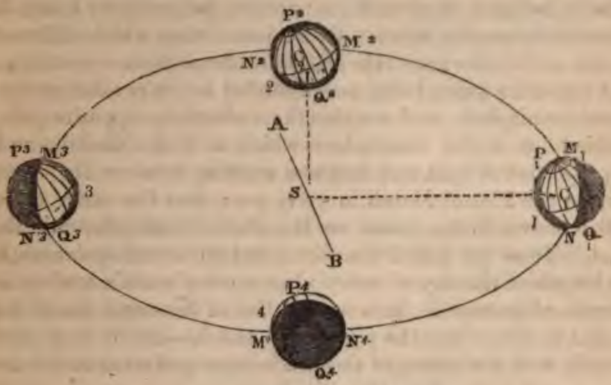
The earth is a sphere composed of opaque materials: that hemisphere which is presented to the sun is enlightened and heated; and the opposite hemisphere is in the shadow, in cold and darkness. Being a sphere, the different portions of its surface are presented with different degrees of obliquity to the action of the sun's rays; and were the axis about which it revolves always in the plane which separates its enlightened and darkened hemispheres, or rather, did that plane always pass through its axis, then its revolution upon its axis would not in any way modify that unequal distribution of temperature which results from this difference of obliquity. Also, were the position of the boundary of the two hemispheres *fixed* in reference to the axis, supposing it *not* to be in the plane of that boundary, any accession of temperature thus given to certain oblique portions of the surface could only be so given at the expense of others. But in reality the position of the boundary of the dark and enlightened hemispheres is continually changing in respect to the earth's axis; and thus is brought about that distribution of temperature on its surface which is perhaps the most equable that can be conceived. The seasons are no more than a contrivance by which the unequal distribution of temperature resulting from the unequal obliquity of that portion of the earth's surface which is presented to the sun is equalized. This will be seen at once, when the way in which they are brought about has been explained.

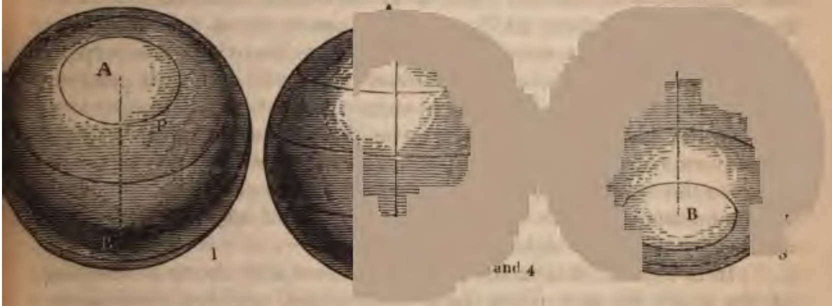
The pole of the heavens always throughout the year, and from year to year, appears to retain the same place in the heavens, except that there is an exceedingly minute secular variation of its position, called precession, and hereafter to be explained. Now, the pole of the heavens is the point in which the axis of the earth, when produced, in any of its positions, meets the heavens. Hence, therefore, it follows that the axis of the earth, in one of its positions in its orbit, and its axis in any other, include, when produced to the sphere of the heavens, a space which is imperceptible to us on the earth's surface. And from this it follows that its axis in one of these positions must be parallel to its axis in the other; for if they were ever so little inclined, being produced so infinitely far as the region of the fixed stars, they would include a perceptible space. In any one position of its orbit, then, the position of the earth's axis is parallel to its position in any other. This is usually expressed by saying, that in its annual motion the position of the earth's axis remains parallel to itself.

Not only is this a fact given us directly by observation, but it is also one resulting from the laws of mechanics, as applied to a material body, placed under the circumstances in which we know the earth to be placed.

Did no force whatever act upon it besides that which was *impulsively* communicated to it in the beginning, and from which has resulted its motions of rotation and tangential translation,—it is certain that moving continually forward in the same straight line in space, it would also revolve continually about the same axis within itself, and that this axis would retain its parallelism. Also, if in addition to the first impulsive force, we suppose any other force to be applied to it precisely in the middle of its axis, or precisely through its centre of gravity, it is manifest that this force will have no tendency whatever to alter that parallelism, its effect on either side of the centre of the axis being the same. If, then, the attractive power of the sun, by which the earth is *deflected* from its rectilinear path, acted precisely as though it were applied to its centre only; then would it not in any way tend to alter that parallelism of the earth's axis, which would have resulted from the impulsive force alone. Now it does *very nearly* so act, and would do so *accurately*, were it not for the slightly spheroidical shape of the earth; and from that slight deviation of its form from a sphere, results the minute deflection of its axis, from east to west, which is called precession. By the operation of this force its pole is made to describe a circle in the heavens of 23° 28′ in radius, describing annually 50″·1 of that circle, and thus completing its revolution in 25,868 years.

The axis of the earth remaining throughout its annual motion thus parallel to itself, let us suppose the accompanying figure to represent four of its positions, determined as follows:—Let A B be drawn through the sun *s*, parallel to the directions of the earth's axis, in all its positions. Through A B draw a plane perpendicular to the plane of the earth's orbit, and let c_1 be the centre of the earth, when, by its revolution, it is brought into this plane. Through C_1 draw a plane $M_1 N_1$ perpendicular to SC_1, and this will be the boundary of light and darkness. Draw $P_1 Q_1$ parallel to AB, and it will be the earth's axis. Take SC_2 perpendicular to SC_1, and let C_2 be the position of the earth's centre, when, in its revolution, it passes through the line SC_2,—draw the plane $M_2 N_2$ perpendicular to SC_2,—then will it be the boundary of light and darkness; and if $P_2 Q_2$ be drawn parallel to AB, it will be the earth's axis. 3 and 4 represent the positions of the earth determined as the above, but on opposite sides of S. Now it is evident that in the positions 1 and 3, the earth's axis, $P_1 Q_1$, $P_3 Q_3$, is more inclined to the boundary of light and darkness, $M_1 N_1$, $M_3 N_3$, than in any of its other positions*. Also the line SC_2, being perpendicular to SC_1, and the plane $M_2 N_2$ perpendicular to SC_2, it follows that $M_2 N_2$ is parallel to the plane ASC_1; and, therefore, that $P_1 Q_1$, which is parallel to AB, is in the plane $M_2 N_2$; and the same is the case in the position 4 of the earth. All these relative positions of the earth's axis, and its boundary of light and darkness, are shown on a larger scale in the accompanying figures.





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* This will be readily understood if we imagine planes to be drawn through S, parallel to the boundary of light and darkness of the earth, *in all its positions; these planes will manifestly be inclined to* AB, precisely as the corresponding boundary plane is to the earth's axis. Also the greatest inclination will manifestly be that of the plane perpendicular to SC_1.

In the positions 1 and 3, the earth's axis is inclined, at its greatest angle, to the boundary of light and darkness, and its poles are at their greatest distance from that boundary. Also, in the positions 2 and 4, the earth's axis is actually in the boundary of light and darkness. Thus, in the positions 2 and 4, every part of the earth is kept by its revolution as long in the shadow as in the light, and imbibes heat at every point of its surface, during precisely the same period of each revolution that it radiates, or gives it out; and in the positions 1 and 3, there is the greatest inequality between the periods during which different parts of the earth receive the sun's rays and are without them,—certain portions of it near one of its poles, being never carried by its revolution out of the enlightened hemisphere, and certain others about the opposite pole, being never carried out of that hemisphere which is in the shadow, and every possible proportion of light and darkness existing between these extremes.

It is on the 21st of March in every year, that the earth comes into the position shown in fig. 2, and on the 21st of September into the position, fig. 4. These are called the vernal and autumnal *equinoxes*, because then the length of the day of every place on the earth's surface is equal to the length of its night. It is on the 21st of June and the 21st of September, that it comes into the positions 1 and 3,—and it is at the first of these periods that the action of the sun is most powerful in this northern hemisphere, and at the other that it is least.

In position 1, or about the 21st of June, a considerable portion of the earth, near the north pole, is never by its revolution carried out of the enlightened hemisphere, so that here there is the continual warmth and heat of the day. The apparent elevation of the pole of the heavens has been shown to equal the latitude: now the latitude of these polar regions approaches 90°, so that there the point about which the whole heavens appear to turn is nearly in the zenith. Thus the sun appears to the inhabitants of the arctic circle, about the middle of our summer, to describe, with the rest of the heavens, a circle round the horizon, at an elevation which increases continually from the 21st of March, until, on the 21st of June, it attains to about $23\frac{1}{2}^\circ$. On the contrary, about the region of the south pole, or, as it is termed, the antarctic region, there is then a perpetual night, the whole of the earth's surface, within $23\frac{1}{2}^\circ$ of that pole, continuing as it revolves *without* the boundary of light and darkness.

In the middle of our winter, or on the 21st of December, matters are reversed, as shown in fig. 3. The enlightened hemisphere now includes the south pole, and the north is immersed. The inhabitants of the antarctic regions have now perpetual sunshine, and those of the arctic have perpetual night.

It is evident that in the position 1, there is an excess of the length of the space through which each point of the northern hemisphere is carried in light, over that through which it is carried in darkness; that is, there is an excess of the length of the day over that of the night; whilst in the southern hemisphere there is the opposite proportion. Also, in the position 1, the sun's rays fall more perpendicularly on the northern hemisphere than in any other position. For both these reasons, then, because of the excess of the time of each place receiving heat over that during which it gives it out, and because of the less obliquity of its incidence, we have in this position of the earth *summer* in our northern hemisphere, whilst in the southern there is the *contrary* of all this, and consequently winter.

As the earth moves from position 1 to position 2, the excess of the day over the night diminishes, until at position 2, that is, on the 21st of March, it vanishes, and there is an equality, or it is the equinox. From 2 to 3, the night of the northern hemisphere continually gains on the day, and the difference is at 3, on the 21st of December, the greatest. From 3 to 4 it diminishes, and vanishes again at 4, which is the autumnal equinox.

VII.

Solar Time.—Sidereal Time.

With regard to the annual motion of the earth, one of the first things which it is important to remark is,—that whilst it is revolving from one point in its orbit to the same point again, it does not make a complete number of revolutions. Thus, if at the instant when the meridian of any place is passing over a fixed star, the precise place of the earth in its orbit be ascertained, then, at the instant when it has returned to the same place in its orbit, that meridian will not be passing over the same star as it would be if the earth in the interval had made a complete number of revolutions,—the revolution which it last commenced will remain uncompleted.

The number of complete revolutions which the earth will have made is 366; and of its 367th revolution it will have described that portion which it occupies 6 9′ 9·6″ to describe. Now we usually say that there are 365 days and a quarter in a year, and each day is produced by a revolution of the earth upon its axis; how is it, then, that the number of revolutions is thus greater by one than the number of days? This will readily be understood by referring to the figure, p. 315 of our last number. Let A, B, C represent successive positions of the earth in her orbit, S the sun, and AP, BP, CP lines drawn from the centre of the earth to a fixed star, which are to be considered parallel to one another, because of the distance of the star. Suppose the meridian of any place on the earth's surface to pass over this star, and the sun at the same instant in the position A. In the position B, the star will appear at N and the sun at K, and the meridian revolving in the direction KNB, will have to describe the angle NBS, after passing over the star, before it can pass over the sun; but when it passes over the star it will have completed a certain number of revolutions exactly from the time it left the position A; that is, it will have completed a certain number of sidereal days; and when it passes over the sun it will have completed a certain number of solar days exactly: a certain number of revolutions, or sidereal days, is, therefore, completed before the like number of solar days, or alternations of day and night, is completed.

Now, the angle NBK, which the meridian has to describe af completing *a certain number* of sidereal days, before it completes *like number of solar* days, is equal to the angle ASB which the has, in the mean time, described about the sun; when, therefore, the earth has completed its revolution about the sun, or described 360°, the angle which the meridian has to describe, after completing a certain number of sidereal days, before it completes the same number of solar days, is 360°; but this 360°, being a complete revolution, will take it just another sidereal day to describe, which will make the whole number of sidereal days one more than the whole number of solar days.

VIII.

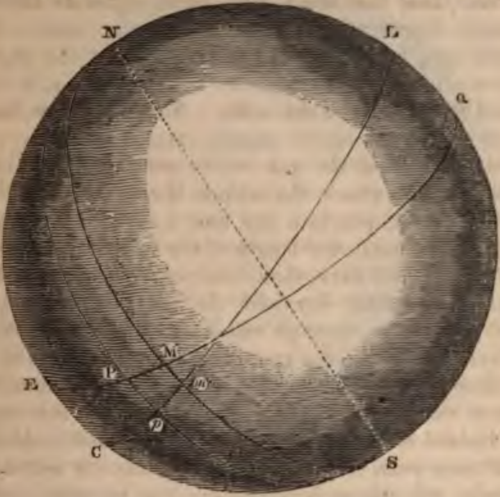
The Divisions of Time.—True Time.—Mean Time.

That division of time which is most obviously presented to us by the phenomena of the heavens, and which, as long as the world lasts, will continue to be the great practical division of time, is the alternation of a day and a night. This great division was established in the beginning of things, when God first divided the light from the darkness, and "the evening and the morning were the first day." But a very slight observation is sufficient to show us, that the length of the period of light, and the length of the period of darkness, is perpetually varying,—that, for instance, the day of summer is longer than the day of winter, and that an opposite relation obtains with regard to the nights of these two seasons; but that the sum of these two periods, is, all the year round, and all the world over, nearly the same. This sum, which is nearly, and was at first imagined to be exactly, uniform, was called a day. Thus we understand the full force of the expression, "the evening and the morning were the first day." The first measurement of the length of the time of light and the length of the time of darkness, was no doubt made by observing the time between sunrise and sunset, and between sunset and sunrise; and this method admits of considerable accuracy. It would soon, however, be found to be at once more convenient and more accurate to observe the interval between two apparent passages of the sun over the meridian, or two of its greatest successive elevations in the heavens. Before instruments applicable to the exact admeasurement of angles came to be used, this was done by observing the interval between the times on two successive days when the length of the shadow of a vertical object was least.

This period is the true solar day. It is divided into 24 equal parts, called hours; and if these be counted from noon up to noon again, it is the astronomical day.

Now observations of so rough and uncertain a kind as those spoken of above, are yet sufficient to establish the fact that this solar day is not constantly of the same length. This irregularity of the length of the solar, as compared with the sidereal day, arises principally out of two causes. The first is, that the sun's *apparent* path round the earth is not parallel to the apparent paths of the stars,—or, in other words, that the axis about which the sun apparently revolves round the earth every year (the axis of the ecliptic,) does not coincide with the axis about which the earth revolves every day. The second cause is the continual variation of the motion of the earth in its elliptical orbit.

Let NCSL represent the sphere of the heavens, EQ the equinoctial, L the ecliptic, N, S the poles, *p* the place of the sun in the ecliptic, at the ime when the celestial meridian NPS of any place is passing over it, *m* he place of the sun in the ecliptic on the *following* day. To pass over the un on this following day, the meridian, after completing its revolution nto the position NP*p*S, must further revolve through the angle PNM into

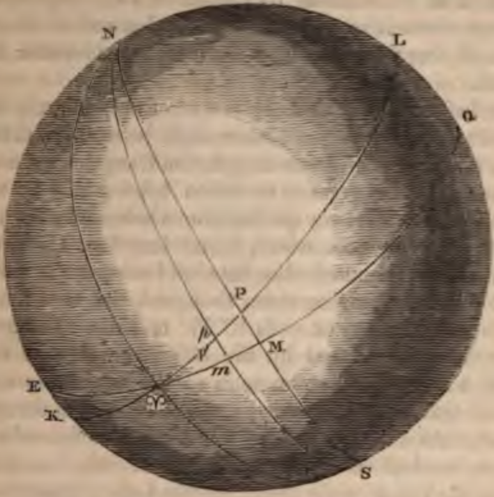


he position NMS; now the meridian revolves *uniformly;* if, therefore, the ngle through which, in order to overtake the sun, it has to revolve every ay, over and above a complete revolution, be the *same*, then will the ength of time between its leaving the sun and returning to the sun gain be the *same*,—or, in other words, the solar day will be always f the *same* length; but, on the contrary, if this angle be *not* always the ame, the lengths of successive solar days will, for this cause, be different. Now, supposing the sun to move *uniformly in the ecliptic*, it is manifest hat this angle cannot always be the same, because the ecliptic is *oblique* o the equator.

It is manifest that as the meridian revolves uniformly, it would carry point fixed upon it *uniformly;* and if such a point were fixed upon it, alf way between the poles, it would carry it along the *equinoctial*. The eridian traverses, therefore, the equinoctial *uniformly*, and equal spaces n the equinoctial are revolved over in equal times by the meridian, or orrespond to equal angles described by the meridian; if, therefore, equal paces on the ecliptic corresponded to equal spaces on the equinoctial,—hat is, if taking distances *anywhere* on the ecliptic, each equal to one nother, and to *p m*, the spaces P M on the equinoctial corresponding to hem were all of necessity equal to one another, then the corresponding ngles PNM would all be equal; and if *p m* were the space described by he sun in the ecliptic every day in the year, then would every solar day e of the same length. But this is not the case. If equal spaces, such as *m, be taken on different* points of the ecliptic, it will be found, and it *manifest, that the spaces* such as P M, corresponding to these on the equinoctial, are not equal,—the angles PNM corresponding to equal motions of the sun in the ecliptic are, therefore, *not* equal; and the solar day is not then, at all periods of the year, of the same length, and would not be, even if the sun's motion in the ecliptic were regular. But the sun's motion in the ecliptic is not regular, because the earth's motion in its orbit is not regular. Referring to the fig. in page 315 of the last number, we perceive, that the angle NBS being equal to the angle ASB, if the latter angle, representing the earth's angular motion in any given time about the sun, be not always the same, then the angle NBS or the arc N K, representing the sun's apparent angular motion in the ecliptic in that time, will not always be the same. Now we know, and it will be shown hereafter, that the earth's angular motion about the sun is varied, because its distance from the sun varies continually. Thus, then, the irregular motion of the sun in the ecliptic is accounted for; it sometimes describes $57'$ of the ecliptic in a day, and sometimes $61'$; and from this cause arises a difference in the length of the solar day which may amount to $8' 20''$ of time. We have, then, two principal causes of irregularity, in the length of the solar day, and the true time of noon. 1st. The inequality of the angles through which the meridian must revolve on successive days to overtake the sun, caused by the obliquity of his path. 2dly. The irregularity of his motion in his path, resulting from the elliptic form of the earth's orbit. If we imagine a sun to traverse the equinoctial instead of the ecliptic, with a continued *uniform* motion in the period of each year, or in 365·2,422,414 days, it will describe an arc of $59' 8\frac{1}{3}''$ every day, through which arc the meridian will revolve in $3' 56\frac{1}{2}''$ of sidereal time. If, therefore, P be the position of such a sun on one day, and M, at a distance $59' 8\frac{1}{3}''$ from it, be its position on the next, then will the meridian NPS arrive at M, $3' 56\frac{1}{2}''$ after completing one entire revolution of the heavens. If, therefore, we take a pendulum clock, and so regulate the length of its pendulum, that its hour-hand shall have completed $3' 56\frac{1}{2}''$ short of one entire revolution, in the period of one entire revolution of the meridian, as marked by two passages of the meridian over the same stars, then, $3' 56\frac{1}{2}''$ after this the meridian will pass over our imaginary sun, and, at the same instant, the hand of the clock will have completed its revolution. A clock thus regulated is said to be regulated to *mean* solar time.

Now let us suppose that our imaginary sun sets out from the point Aries, ♈, (see the figure on the next page), at the instant of the vernal equinox, when the true sun is also in that point. Let the dial-plate of the clock be divided into 24 equal parts, and let the hand at that instant stand at 24. Also let the meridian N ♈ S be at that instant passing over the sun. Let m be the position of the imaginary sun at the instant when the hand of the clock next points to 24, and the meridian is again passing over the imaginary sun. Since the angle ♈ mp is a right angle, ♈ p is the hypothenuse of a right-angled triangle, and is therefore greater than ♈ m. The true sun having, therefore, described in the ecliptic a space equal to that of the imaginary sun in the equinoctial, will be at some point p' in ♈ p such that ♈ p' = ♈ m. Thus, then, when the meridian passes over m, it will have first passed over p', or it will have passed over the true sun before it passes over the imaginary sun, or before the hand of the clock again shows 24 hours.

Thus, about the equinox the time of *true* noon precedes the time of *mean* noon, by reason of the excess of the space ♈ p described in a mean solar day by the true sun, over that ♈ m described by mean sun. For some weeks the time by which the *true* thus precedes the *mean* noon will continue to increase, until it has attained an interval of about 16′; it will



then continually diminish until at the solstice P it vanishes; for it is manifest that there the corresponding arcs of the ecliptic and equinoctial are equal; so that supposing, as we have done, that the true and mean sun move each with the same uniform velocity, the meridian will pass over them both at the same time. True and mean noon coincide therefore at the solstices. After the solstice is passed, mean noon will begin to precede true noon, and the interval will again increase up to a certain point between this solstice and the following equinox; having then attained its maximum, it will begin to diminish, until at the equinox it vanishes, and mean and true noon again coincide. In passing on further to the next solstice, the time of true will begin to precede that of mean noon, and the same changes will be gone through as in the preceding half of the ecliptic, until both suns again come together, and both noons coincide in the point Aries ♈, whence they set out. Thus, then, on the supposition which we have made, that the sun moves *uniformly* in the ecliptic, it appears that the time of true and mean noon will alternately precede one another, and that four times a year the interval between them will attain a maximum value*.

The sun does not, however, move uniformly in the ecliptic, by reason of the ellipticity of the orbit of the earth; and, moreover, the velocity of his apparent motion is dependent, not upon his position with respect to the solstitial or equinoctial points, but upon the position of the earth with respect to the principal points of her orbit about him, her aphelion and perihelion, the nearest and most distant points. Thus, then, the amount of the deviation of the motion of the sun, at any point of the ecliptic, from his mean motion, is dependent on the position of the perihelion of the earth's orbit in the ecliptic; moreover, this position is varying from year to year. Here, then, is another and most important cause of the variation of the time of true from that of mean noon, by reason of which cause alone it may be calculated, that at certain periods of the year the time of true noon would differ from that of mean noon by about 8′ 20″ of time.

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* *This maximum value will be attained* when the sun is 46° 14′ from either equinox, *and it may amount to* 10′ 3·9″ *of* time.

It has been before stated that, by reason of the obliquity of the ecliptic alone, the times of true and mean noon might be made to differ 10′ 3·9″ of time. If, then, the time of greatest variation from the one cause coincided with the time when the greatest variation takes place by reason of the other cause, then both thus conspiring, the whole variation of the time of true from that of mean noon would be not less than 18′ 23·9″. But this is not the case,—and the maximum interval between the time of noon as shown by a good clock keeping mean time, and the true time of the sun's passing the meridian, never exceeds 16′ 17″ of time. Moreover, by reason of the irregularity introduced by the elliptic motion of the earth, the coincidence of the true and mean noon at the equinoxes and solstices is destroyed, and true noon is shown by the clock, not at the periods of the equinoxes and solstices, but at the following periods,—the 15th April, the 15th June, 1st September, 24th December. The sun gains upon the clock between the two first of these periods, loses during the second, and gains again during the third. It is *behind* the clock by its greatest interval of 14′ 37″ on the 11th of February, and before it by its greatest interval of 16′ 17″ on the 3d of November.

There are three methods of measuring time, commonly in use among astronomers.

1. It is measured by sidereal time, which is regular, being governed by the regular revolutions of the earth upon its axis, as shown by successive returns of the meridian to the same star.

2. It is measured by *mean* time, the nature of which has been sufficiently explained in the preceding pages, and the method of regulating an astronomical clock, so as to show that time (see p. 358); this time, like sidereal time, is *uniform*, being dependent upon the period required by the earth to make one complete revolution in her orbit.

3. Solar, or *true* time, as it is called, which is measured by the time between two successive noons, or actual passages of the meridian of any place over the sun; and that time not being the same at all seasons of the year, it follows that solar time is *irregular*, and that the solar hour, which is the 24th part of the solar day, has not exactly the same length on any two successive days.

The difference between true and mean solar time, explained in the preceding pages, is called the equation of time. Clocks, called equation-clocks, have been so constructed, that whilst one of their hands shows on the dial-plate mean time, the other points to true time. The mechanism of a clock, whose hand is to follow the irregular course of the sun, through each quarter of the year, is, however, so complicated, that little dependence can be placed upon it.

The sidereal day, which, like the solar, is divided into 24 hours, commences at the instant when the meridian, at the place of observation, passes over the equinoctial point Aries, and terminates when it returns to that point.

Thus the time of the sidereal day, when the meridian passes over any particular star, is the time which it takes to revolve from the equinoctial point Aries to that star; and since it revolves regularly through 15° in every sidereal hour*, it is manifest that, allowing at the rate of 15° for each sidereal hour shown by the clock, (or, as it is called, converting the time into degrees,) we may ascertain at once the right ascension of the star, which is no other than the number of such degrees intervening between it and the point Aries, by observing the sidereal time when the meridian passes over it.

Since the right ascensions of all the principal stars have been accurately ascertained; by observing the sidereal time shown by the clock when the meridian passes over any such star, we may conversely ascertain whether the clock be right or not; and it is thus that astronomical clocks are regulated.

Solar time is found by observing the time of two successive passages of the sun over the meridian, and dividing the interval into 24 hours. It is the time shown also by a well-constructed sun-dial.

Mean time is found by observing the *true* time, and allowing, according to the table of equation of time, for its difference from true time. Thus, to determine the mean time of noon, we should *observe by our clock* the true time of noon, or the exact time of the meridian passing over the centre of the sun. If then we deduct from this, or add to it, the equation of time for the noon of that day, the result will bring us to mean noon.

There is yet another, and practically a better method. If a clock be set to true mean time, the stars will every day complete an apparent revolution,—that is, the meridian itself will complete a *real* revolution,—precisely 3′ 55·9″ before the hour-hand has completed its revolution of 24 hours on the dial. Observe, then, two successive transits of a star; at the first set the hour-hand at 12, and regulate it so that at the second it shall show 3′ 55·9″ short of 12. It will then be regulated so as to show mean time. It only remains to *set* it at the mean noon, as explained in the preceding page.

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* This is evident from the fact that it completes its revolution of 360° in 24 sidereal hours.

A POPULAR COURSE OF CHEMISTRY.

VII.

Gases.—Hydrogen.

It is now my intention to introduce to your notice a most remarkable and highly-interesting gaseous element, namely *hydrogen*. Before, however, you can succeed properly in evolving it, there are several manipulations requisite to be performed, which are as follows:

In the first place, you must obtain a pound or two of the metal *zinc*, and melt this in an iron ladle over a bright fire, just as you would so much *lead*. When perfectly melted, pour the metal very slowly from a height of three or four feet into a large pailful of cold water; it will hiss and spatter a good deal, but you need not be alarmed at that, for there is no danger. It is best not to hold the ladle so that all the melted zinc falls exactly in the centre of the water, but to carry the ladle round and round, so that the melted metal may be more dispersed in its fall. Now having done this, empty the water from the pail, and you will find at its bottom the zinc in small irregular pieces, of various sizes; collect them together (and be careful how you do this, for some of them are very sharp, and will wound the fingers very severely), and place them on a sieve, or in a large funnel to drain.

The process of thus reducing zinc into small particles is called *granulation*, and the result, *granulated zinc;* it is a very convenient form of the metal for many operations. You would find it a matter of no small difficulty to break up a mass of zinc by mechanical means,—with a chisel and hammer it would take you a long time; but here, by the chemical agency of heat, you destroy the cohesion of the metal in a few minutes; and by pouring it in the melted state slowly into water, it solidifies into small masses. This operation of *granulation* is not confined to zinc alone; many other metals may be similarly treated, as will be fully shown hereafter.

The *granulated zinc* must now be dried by spreading it on the hob of the grate, and whilst this is doing, you may proceed to another operation.

Select a soft, sound, and accurately-cut cork, that will tightly fit a narrow-mouthed pint glass bottle, and with a " rat-tail" file or " rasp" make a circular hole very neatly through the cork, so as to admit one end of a foot of common " gas-tubing," either of pewter or copper. If this does not happen to fit as accurately as you could wish, melt a little bees'-wax around it; the tube must now be bent into a shape something like the letter S turned thus, ∽. A little knack is required in thus bending the tube so as to make two regular and handsome bends; for if you do not mind what you are about, you will get a very ugly shape, flat in some parts, and round in others, on account of the unequal strain upon the metal; but if you stop up one end of the tube with a small cork, then fill it full of dry sand, and stop up the other end also with a cork, you render the tube much more manageable, because as you bend it, the enclosed sand resists any unequal pressure, and supports all parts of the metal alike, and thus you obtain a very neatly bent tube; now remove the corks and shake out the sand. Put an ounce or two of *granulated zinc* into the bottle, and about half fill it with *water*; and having arranged your pneumatic trough, and all its bottles and jars with cold water, as directed for *oxygen*, pour into what we will call the gas-bottle, that is, the glass bottle, some strong *sulphuric acid* (oil of vitriol), until you find a tolerably brisk *effervescence* ensue; then insert the curved pipe; wait a minute or so before you plunge its extremity under the water of the trough, in order that all the common air may be thoroughly expelled by the *hydrogen* which is evolving, for the effervescence is due to its liberation. Then proceed to collect it, by causing it to bubble through the water, as for *oxygen* or *chlorine;* you will find it come over very readily and very abundantly; but should the action flag after a time, empty out the liquid contents of the "gas-bottle," leaving only the zinc, and insert a fresh charge of water and acid as before, and thus you may go on evolving *hydrogen* until all the zinc is entirely dissolved. Now you find that the *hydrogen gas* collected in your pneumatic apparatus is as perfectly invisible as *common air* or *oxygen;* but perhaps during the operation of collecting it some has escaped, and you have observed a peculiar smell; this, however, is owing to impurities in the *zinc* employed in the experiment; perfectly-pure hydrogen is devoid of smell, but it is a most difficult matter to obtain it. However, what you have collected is quite pure enough for all your experiments; and now proceed to examine its properties.

Open a bottle of the gas, and immerse a lighted taper,—the *hydrogen* instantly takes fire, with a very pale flame, so pale, indeed, as scarcely to be visible in broad day-light; it is therefore an *inflammable gas;* but you will remark that the taper is extinguished,—so that *hydrogen* although *inflammable*, and highly so, as subsequent experiments will show, yet will not *support combustion.* You will have a better opportunity of examining the flame of hydrogen if you make a hole through another cork to fit the large end of a long piece of tobacco-pipe, and placing this in the neck of the gas-bottle, after putting in a charge of *zinc*, *water*, and *acid;* allow the effervescence to proceed for about two minutes, and then apply a bit of lighted paper to the other end of the tobacco-pipe, the hydrogen will instantly take fire with a sharp pop, and burn for a very long time with a beautiful pale flame. This arrangement constitutes "Priestley's philosophical candle;" rather an unlucky term by the way, because, although a "*philosophical*," it is by no means a *luminous* candle, being the purest and *palest* form of flame hitherto known. Place it in a dark room, and you will barely be able to distinguish the printed letters of a book even when held very close to it; but although thus non-luminous, it is most intensely hot; it will readily kindle a bit of paper or wood, and heat a bit of iron to a very high temperature, or if you happen to have a bit of thin platinum-wire amongst your "chemicals," hold it in the pale flame and remark how intensely the wire is *ignited*, not burned, remember, and how much *light* is now suddenly evolved, by the introduction of this *solid*, and almost infusible *and incombustible metal.* The book actually becomes legible now; but *remove the platinum*-wire and it is no longer so.

All these facts regarding the increased light of pale flames by the introduction of solid bodies, will come before you on a future occasion; and therefore I do not enter upon them in detail at present.

Now, in the first experiment, with the bottle of hydrogen in the pneumatic trough, and also in the second, with the "philosophical candle," the gas took fire with only a very slight pop, scarcely meriting the title of an explosion, because it was tolerably pure and free from admixture with common air; but supposing this to have been present, what result would have taken place? why, the hydrogen, instead of burning *quietly*, would have burned with *explosion*.

Let us take the "philosophical candle," to illustrate this point. You find it burning quietly on, because the combustion is supported by the air around, which comes gradually around to answer the demand of the flame for *oxygen*, which, as I have frequently told you, is one of the constituents of air; but, supposing that we applied a light to the end of the tobacco-pipe, the instant after the *acid* was poured on the *water* and *zinc*, what would have been the consequence? why an explosion,—because the materials only occupying about one half of the bottle, the other half was filled with air; and when the first portions of hydrogen were evolved, mixing with this they had *oxygen* enough to support their combustion, not slowly, but rapidly, and the whole arrangement would have been blown to pieces by the explosion.

Hence, in experimenting with hydrogen, you should always be cautious to let the common air be completely expelled before you attempt either to collect or inflame the gas, otherwise a serious accident may happen to you, by the fragments of the glass apparatus being violently scattered around by the explosion.

You can safely make an experiment which will satisfy you regarding this matter, by taking a bladder rather more than half full of air, and then tying it on to the bent-tube of the gas-bottle, or the pipe of the "philosophical candle," until its inflation is completed with hydrogen; then, removing it from the tube, hold a lighted taper to its neck,—the mixture of *air and hydrogen* will explode with great violence, rending the bladder into threads.

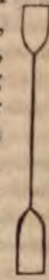
Take a tin tube, about two inches in diameter and a foot long, closed at one end; fill it with water in the pneumatic trough, as you would any other vessel; then transfer into it *six* parts or small glasses full of *air*, and *two* of *hydrogen*,—apply a light to this, you get a loud explosion, and there is no danger of any accident happening. Do not attempt the experiment in a glass tube of similar size.

Supposing that you again take this tube and transfer into it *one* measure or part of *oxygen*, and *two* of *hydrogen*, and put a lighted taper to this mixture, you will obtain a yet more rapid and powerful explosion, because the *oxygen* being *pure*, and unmixed with *nitrogen*, as in atmospheric air, the hydrogen combines with it far more intensely and eagerly.

Next to the inflammability of hydrogen, its *levity* is the most remarkable character, and we will now proceed to examine this.

Take two bottles of hydrogen, place one of them on the table, with *its mouth upwards*, and remove the stopper; hold the other with its mouth *downwards*, and remove its stopper; let the bottles remain in this position for about a minute, and then apply a lighted taper to the mouths of each; you will find no flame issue from that with its mouth *upwards*, but a flame and explosion from that with its mouth *downwards;* both bottles were full of hydrogen at the outset of the experiment, therefore, what has become of it out of the one bottle? Why, the *light hydrogen* has *ascended* into the *air*, or, if you please, the *heavy air* has *pressed* it out of the bottle; but the hydrogen remains in the bottle held with its mouth *downwards*, because, being so much lighter than air, it cannot fall through it, nor can the heavy air force it out; a little mixture of the air and hydrogen certainly does take place, and that causes the explosion when you apply the taper; and ultimately the hydrogen would all escape, in virtue of a singular tendency which all gaseous bodies have to mix, however opposite may be their relative weights or specific gravities, as will appear from the following experiment:

Take a glass tube, about eighteen inches long, and an eighth of an inch in the bore, open at both ends; perforate two corks that will fit the necks of two stout narrow-mouthed four ounce bottles, and thrust a cork on to each end of the tube; now fill one bottle with *hydrogen*, and the other with *oxygen*, and connect them with the tube and corks, so as to stand like the annexed figure, the lower bottle containing the *oxygen*, and the upper the *hydrogen;* leave them thus at rest for some hours, and then uncork them, and by a lighted taper test them both; you will probably find the contents of each explosive, and certainly so if you leave them connected for a day or two. This proves that the *heavy oxygen* has *ascended*, or been attracted upwards by the *light hydrogen*, and *vice versâ;* or otherwise, how could each bottle contain a mixture of the two gases?



This is a very interesting experiment, and well worth making; I should tell you that in corking the bottle, containing the *hydrogen*, you must hold it with the mouth *downwards;* and that containing the *oxygen* with the mouth *upwards*.

The experiments relative to the levity of hydrogen are very numerous, and very beautiful, and it is difficult to make a selection of the most striking. You probably very well know that balloons were generally inflated with *hydrogen*, previous to the discovery of the levity of *coal-gas*, or *carburetted hydrogen;* you may imitate the inflation of a balloon on a miniature scale, by obtaining, from the philosophical instrument makers, a small balloon, made of membrane, about six or eight inches in diameter; these are now very commonly sold at half a crown, and from that to five or six shillings each; they are excessively light, and if you press all the air out of one of them, and then tie it on to the bent-tube of the gas-bottle, or the pipe of the philosophical candle, you can easily inflate it with hydrogen, and then upon detaching it from the pipe, it will be found so buoyant as to rise rapidly to the ceiling of the room, where it remains until the hydrogen escapes through the pores of the membrane, and then your miniature balloon falls to the floor.

When this experiment was first shown to a party of philosophers, (I *believe by Dr. Black,)* they, being unacquainted with the extreme levity *of hydrogen, could not* believe that the thin membrane or bladder rose and remained at the ceiling of its own accord; they suspected that some confederate was in the room above, who, at a given signal from below, drew up the thin bladder, by means of a fine horse-hair or silk thread, attached to it, and passing through a small hole in the ceiling!

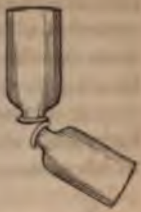
Balloons of enormous magnitude, inflated with hydrogen or carburetted hydrogen, are now so common, that almost everybody is aware of the cause of their ascending force, and no one suspects that they are pulled upwards by any threads or strings. But, although thus common, a balloon ascent never fails to excite a vast deal of attention and interest; indeed, I think it a most magnificent sight to see a huge balloon bound away from the earth, and soar away amidst the regions of the air.

Hitherto the balloon has not proved a very useful instrument to science, although some curious facts and observations concerning the state of the higher regions of the atmosphere have certainly been contributed to our stock of knowledge through its agency; and we cannot help looking forward with some considerable degree of interest for an authentic and detailed account of the magnificent voyage of 480 miles, lately made by three aëronauts in the stupendous "Vauxhall Balloon."

But to return more immediately to the miniature experiments of the laboratory: there is another way of showing the levity of hydrogen, both beautiful and instructive. Make a basinful of strong soap-lather, just as if you were about to blow common soap-bubbles; (an amusement, by the way, practised by the immortal Newton, and therefore demanding no apology for its apparent simplicity or childishness.) Then fill a large bladder (fitted with a stop-cock) full of hydrogen, from either apparatus already described, and to the stop-cock adjust a short tobacco-pipe; immerse this in the lather, then open the cock, compress the bladder gently, and you will succeed in blowing a soap-bubble with *hydrogen;* detach it from the bowl, and away it will soar into the air with great rapidity: this is another miniature balloon. But you will very probably say, "Why, any common soap bubble, blown by a child will do the same thing." Yes, it certainly will ascend, but with nothing like the same rapidity; and the reason that it ascends at all is, that the breath from the lungs with which it is blown is *warmer*, *expanded*, and therefore lighter than air; it is an imitation, not of the "*air-balloon*," which ascends on account of its inherent and permanent levity, but of the "*fire-balloon*," which ascends because the air within it is *temporarily* rarefied by exposure to a high temperature. When this cools, the fire-balloon falls,—whereas the "air-balloon," meaning by the term a *gas* balloon, will go on ascending until it bursts by expansion in the higher regions of the air. A fact this, well known to aëronauts, who, as they rise to a great altitude, are obliged to allow a considerable portion of the gas to escape, or otherwise it would expand in the rare regions of the air, and burst the balloon.

I have not space here to enter into more details concerning the art of aërostation, and therefore shall take leave of the subject, and direct your attention to another experiment, illustrative of the levity of hydrogen.

Take a wide-mouthed glass bottle or jar, full of *common air*, and another of the same size filled with hydrogen, hold one in each hand, *with the mouths downwards*, and in this position let an assistant take out



the stopper of each; then place the neck of the bottle of hydrogen close against that of common air, and transfer, as you would do in the pneumatic trough, the *hydrogen* into the bottle of air; thus it will rise through the *air*, displace it, and fill the bottle, as a lighted candle applied to its mouth will show you directly, by an explosion; but if you similarly test the bottle that first contained the hydrogen, you will have no explosion, because *it* is now filled with air, in which, of course a candle will quietly burn.

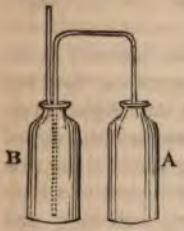
Hydrogen is the *lightest* substance with which the chemist is acquainted,—indeed so light, that formerly its ponderable nature was doubted by some philosophers, for good balances could not detect its weight, even when in very large volume. The chemist, however, invoked the aid of the mechanist, who produced a balance capable of being affected by this highly-attenuated elementary form of matter, 100 cubical inches of which were found to weigh about *two* grains; still further perfecting the balance, hydrogen was found to be yet a little *heavier*, until at the present day its weight is determined with all the accuracy that can be ensured, and it is found that 100 cubical inches weigh 2·118 grains. Such is the perfection of mechanical skill, that some balances are actually affected by the weight of *one cubical inch of hydrogen!*

Besides inflammability and levity, hydrogen has another remarkable property, namely, that of producing *musical sounds* during its combustion, and the mode of experimenting to obtain the result is sufficiently simple. Obtain a common brass blowpipe, heat its smaller end red-hot in the fire, suffer it to cool, and then make it perfectly straight, (you must not attempt to do this whilst it is hot, for *hot* brass is very brittle;) substitute this for the tobacco-pipe in the "philosophical candle," and proceed to evolve hydrogen, which, after the lapse of a minute or two, kindle at the end of the brass tube. You must not have a very rapid evolution of the gas, therefore do not pour in much *acid*, neither let the *zinc* be too small,—indeed a lump or two is better than a number of small pieces, for you do not require much hydrogen, and only want a very small flame. Having succeeded in obtaining this, hold over it so as to enclose it, a tube of almost any material—glass, brass, tin, or copper, about an inch in diameter and eighteen inches or two feet long,—and a singular musical sound is very soon produced. By partially closing the tube at top, or by employing various-sized tubes, a great variety of tones may be produced.

This effect is not, however, peculiar to hydrogen, for other inflammable gases also produce it, but none in such perfection as hydrogen; there is no magic in the matter, for it simply depends on the infinite number of small explosions of hydrogen with the common air, and these succeeding each other with vast rapidity, and being reverberated by the sides of the tube, combine to produce a musical sound.

If, in order to produce a musical sound by the combustion of hydrogen, you happen to hold a glass tube over the flame, you will find its interior very soon covered with a sort of mist or dew; or if you hold a clean, dry, and cold bell-glass over it, although you obtain no sound, yet you still find this deposition of moisture on its interior.

I have now to point out to you the cause of this remarkable appearance, which at first sight you would naturally enough conclude was referrible either to the dampness of the glass, or to some steam arising from the effervescent mixture in the bottle to which the jet is attached. In order to satisfy you that the appearance of the moisture is not accidental, it may be worth while to prepare and burn some perfectly *dry* hydrogen; and this experiment will give you a notion of the way in which some gases are *desiccated*.



Arrange a little apparatus like the annexed figure. Two bottles, each fitted with a cork, perforated so as to allow the tube from A to pass to the bottom of B, from whose cork a straight tube arises: a piece of tobacco-pipe will answer the purpose, or the brass tube already spoken of. Pour some strong *sulphuric acid* into B, so that the end of the bent-tube (which should be of copper or glass) just dips beneath it; you can easily judge how much will be necessary by previous a outside admeasurement; then put in the cork, and make it fit tight with melted wax. Into A put the *zinc*, *water*, and *acid*, and then quickly make its cork tight also. Now you will observe that as the hydrogen evolves in A, the bent-tube conducts it into B, and as it bubbles through the sulphuric acid, should any steam or moisture be hurried over by the heat of the effervescence, it will be absorbed and arrested by the acid, which is exceedingly *hygrometric*, and has a strong attraction for water; *dry hydrogen*, therefore, will now fill B, and issue from the jet, and when you judge that all the common air is expelled from both bottles, you may kindle the hydrogen; hold the dry bell-glass over its flame, you will observe that although the gas is dried, yet its flame still deposits moisture on the interior of the cold glass. The reason why this takes place is as follows. You very well know that a combustible body cannot burn without some *supporter of combustion* being present,—that a candle burns in air because the *oxygen* of the air supports its combustion; such also is the case with *hydrogen;* its flame is in this experiment supported by the *oxygen* of the air; the *hydrogen* and the *oxygen*, therefore, have a mutual *affinity* for each other; they *combine*, and, however strange it may appear, the *result* of the combination is *water*. This is an excellent instance of chemical affinity, and shows you in a most striking manner how completely chemistry is a science of experiment. Who would imagine that *water* consisted of *two invisible gases?* that this, however, is the case, has been incontestibly proved, and it is found that whenever *hydrogen* has its flame supported by *oxygen*, nothing but *water* is the product of he combustion.

Then what is the product of the combustion of a candle, a lamp, or a fire? Why this is rather more complicated, but I must just mention it here, for I think you will understand it. The wax or tallow of the candle, the oil of the lamp, and the coals of the fire, consist of *hydrogen* and *carbon*, both combustible bodies, and when these enter into combustion, the *hydrogen* unites with the oxygen to produce *water*, and the *carbon* with the *oxygen* to produce carbonic acid,—so that when you say that the candle, the lamp, or the fire are burnt out, you, in fact, mean that they are resolved into *water* and *carbonic acid.* Considering the enormous quantities of fuel hourly consumed, either for the sake of its heat or of its light, you may very naturally put the question, what becomes of all the water and carbonic acid? why are we not drenched with showers of the one, or suffocated by the deleterious vapour of the other? For this reason,—because air has the remarkable property of *dissolving* watery vapour, and therefore as it pours into the air, it is rapidly wafted along with the carbonic acid, and so diffused throughout the enormous bulk of the atmosphere, that we never find any inconvenient accumulation of these products of combustion.

To show that *water* is produced by the hydrogen of a burning candle, hold a cold and dry bell-glass over it, and you get the watery vapour condensed on the cold surface directly; now close the mouth of the bell-glass with a card or plate, turn the mouth uppermost; remove the card, and quickly pour in a little *lime-water*, a perfectly clear liquid, but it instantly becomes turbid and milky, upon meeting with the contents of the jar; *lime-water* is a *test* of *carbonic acid*,—it unites with it to form *carbonate of lime* or *chalk*, which is the cause of the turbidness. Try a similar experiment with the flame of *pure hydrogen*,—you only obtain *water*, and the lime-water is not troubled.

The discovery of the composition of water was made by the celebrated Mr. Cavendish, and its composition is now universally admitted to be *oxygen* and *hydrogen*, in the proportions by weight of eight parts of *oxygen* to one part of *hydrogen* = nine parts of *water;* you will remember that eight and one are the *equivalents* of *oxygen* and *hydrogen*, consequently nine is the *equivalent* of *water.* Now there are many other experiments that I might adduce which would show you the *composition* of water *synthetically*, but they require more manipulation and apparatus than I presume you are yet master of; I shall, therefore, content myself with adducing two instructive experiments to show its *decomposition*, or *analysis.* The materials in the "gas-bottle" or the "philosophical candle" apparatus present you with the first; there you have *zinc*, *water*, and *sulphuric acid*, and you evolve *hydrogen*,—but why? Because the *water* consists of *oxygen* and *hydrogen;* the former is attracted by the *zinc*, forming *oxide of zinc*, with which the *sulphuric* acid instantly unites to produce *sulphate of zinc*, whilst the *hydrogen* escapes in the gaseous form. Its source, therefore, is the *water* which the *zinc* is enabled to *decompose* and rob of its *oxygen* through the agency of the *sulphuric acid.* If you wait till all the *effervescence* or evolution of hydrogen ceases, then pour off the clear liquid into an earthen basin, and evaporate it to about one-fourth its bulk, upon letting it cool, you will obtain perfect *crystals of sulphate of zinc*, which is a metallic salt, looking very much like Epsom salt, but is violently emetic and poisonous.

There are other metals that attract *oxygen* from water, simply by coming into contact with it, without requiring the intervention of an acid to call their affinities into play. *Potassium* is one of these: take a globule of it about the size of a pea, and put it in a bit of glass tube about half an inch long, closed at one end. Fill a bottle or jar with water, and invert it *on the shelf of the* pneumatic trough, hold the tube containing *the potassium between* the finger and thumb, so that its aperture may be closed, and then plunge it beneath the mouth of the bottle, keeping it closed until fairly beneath it; then gently withdraw the finger so as to let the water get at the potassium,—you will instantly find a copious evolution of gas ensue, which will rise into the bottle: it looks smoky at first, but agitate the bottle a minute or so, and it becomes clear; then, as usual, test it with a lighted taper, it inflames, in fact it is *pure hydrogen.* The *oxygen* of the *water* combining with the *potassium* to produce *oxide of potassium* or *potassa*, *hydrogen* is evolved and thus collected. *Potassa* dissolves in the remaining water of the bottle, and you can detect it by means of a bit of *turmeric paper*, which it renders *brown*, because it is an *alkaline oxide;* and now that you are convinced of the evolution of *hydrogen*, you can throw a bit of *potassium* on a very little *water* in a cup or glass; the metal *floats* on the water, evolving a beautiful *rose-coloured flame;* and the water being in smaller quantity than in the bottle and trough, a stronger alkaline effect is now manifest on the test-paper, because you will recollect that the small bit of potassium only decomposes a very little of the water, leaving much undecomposed, and capable of dissolving the *potassa;* but of course, if you put a globule of potassium, the size of a nut, into a few drops of water, it would *all* be decomposed, and the *potassa* be left in a solid state. The *rose-coloured* flame in this experiment is due to a little of the *potassium* combining with the nascent *hydrogen*, forming *potassiuretted hydrogen*, which inflames by the violent heat of the chemical action produced by the attraction of the *oxygen* for the principal part of the *potassium.* When you made the experiment *under* water instead of *upon* water, there was simply decomposition of the fluid, and no combustion of the nascent hydrogen, because no oxygen was there *free* to support it.

In these experiments you will remark that we have only succeeded in evolving the *hydrogen* of the water in a *free state*, the *oxygen* having entered into combination with the *zinc* or *potassium;* and in all similar cases of the decomposition of water by metals, the *oxygen* is never evolved in a free state; if you wish to obtain *both gases* from water, you must decompose it by *voltaic electricity;* and the manipulations necessary for this experiment, as well as for the determination of some other facts connected with the history of hydrogen, and especially its combination with *chlorine*, forming *muriatic acid*, will form the materials of my next discussion.

RECENT INFORMATION ON

THE PREVENTION AND DETECTION OF SECRET AND ACCIDENTAL POISONING; PARTICULARLY WITH ARSENIC.

One of the most beneficial victories of practical science is that by which the subtle and invisible agents which may have been used in the secret destruction of human life, are detected, seized, and exhibited. No matter how minute the atom, how mingled, how dissolved, the sagacity and skill of the modern chemist ascertains its presence, separates it from all possible combinations, fixes and exposes it, "palpable to sense."

Too much notoriety cannot be given to this truth. Were the public-spirited vicar of Hatton living*, he would have felt it his duty to have promulgated it from the pulpit. The use of poisons, in this country at least, is now confined to the most ignorant classes; and if the knowledge under consideration were thoroughly disseminated amongst them, almost the only motive to this species of murder would be taken away. A conviction of this truth has recently influenced the Society of Arts, and induced them to depart from the avaricious principle of hoarding up the scientific treasures which may have been intrusted to them until they can be published, either for emolument or vanity†.

In the early part of this year, a paper was presented to this Society, by Mr. James Marsh, of the Royal Arsenal, Woolwich, descriptive of *A Method of separating small Quantities of Arsenic, from Substances with which it may have been mixed.* The merit of this paper was estimated so highly, that the large gold medal of the Society was awarded to the author: and further, so admirable was the simplicity and efficacy of the process, so little the preparation and cost of apparatus necessary to make a most exquisite analysis, and so important to the public interest was the object of the process, that the Society ordered the instant publication of the paper, instead of imprisoning it in the pigeon-holes of the secretary, until the next succeeding volume could be published. Equally impressed with the importance and excellence of the process of Mr. Marsh, we propose to follow up this philanthropic intention of the Society of Arts, and to present it to our readers, satisfied that they will be struck with the beauty of this ingenious and practical application of chemical science. Mr. Marsh introduces the subject by stating that:—

"Notwithstanding the improved methods that have of late been invented of detecting the presence of small quantities of arsenic in the food, in the contents of the stomach, and mixed with various other animal and vegetable matters, a process was still wanting for separating it expeditiously and commodiously, and presenting it in a pure unequivocal form for examination by the appropriate tests. Such a process should be capable of detecting arsenic, not only in its usual state of white arsenic, or arsenious acid, but likewise in that of arsénic acid, and of all the compound salts formed by the union of either of these acids with alkaline substances. It ought, also, to exhibit the arsenic in its reguline or metallic state, free from the ambiguity which is sometimes caused by the use of carbonaceous reducing fluxes. It appeared to me, that these objects might be attained by presenting to the arsenic hydrogen gas in its nascent state: the first action of which would be to deoxygenate the arsenic; and the next, to combine with the arsenic, thus deoxygenated, into the well-known gas called arsenuretted hydrogen. Being thus brought to the gaseous state, the arsenic would spontaneously (so to speak) separate itself from the liquor in which it was before dissolved, and might be collected for examination by means of any common gas-apparatus; thus avoiding the trouble, difficulty, and ambiguity of clarification and other processes whereby liquors, suspected of containing arsenic, are prepared for the exhibition of the usual tests, or of evaporation and deflagration, which are sometimes had recourse to in order to separate the arsenic from the organic substances with which it may have been mixed.

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* The late Dr. Parr. It was in his church, after morning service, that he announced and exhibited to his parishioners, Dr. Carmichael Smyth's celebrated mode *of preventing and destroying* contagion.

† It is this principle which has dictated the notice or request distributed by the Royal Society, with the copies which are granted to a contributor of his own paper. Several other Societies in London are similarly costive.

"I had the satisfaction of finding, on trial, that my anticipations were realized; and that I was thus able, not only to separate very minute quantities of arsenic from gruel, soup, porter, coffee, and other alimentary liquors, but that, by continuing the process a sufficient length of time, I could eliminate the whole of the arsenic in the state of arsenuretted hydrogen, either pure or, at most, only mixed with an excess of hydrogen.

"If this gas be set fire to as it issues from the end of a jet of fine bore into the common air, the hydrogen, as the more combustible ingredient, will burn first, and will produce aqueous vapour, while the arsenic will be deposited either in the metallic state, or in that of arsenious acid, according as it is exposed partially or freely to the air. The former condition is brought about by holding a piece of cold window-glass opposite to and in contact with the flame, when a thin metallic film will be immediately deposited on its surface; and the latter, by receiving the flame within a glass tube open at both ends, which, in half a minute, will be found to be dimmed by a white pulverulent sublimate of arsenious acid. By directing the flame obliquely within side of the tube, it strikes against the glass and deposits the arsenic partly in the metallic state. In this case, if the tube, while still warm, be held to the nose, that peculiar odour, somewhat resembling garlic, which is one of the characteristic tests of arsenic, will be perceived. Arsenuretted hydrogen itself has precisely the same odour, but considerable caution should be used in smelling to it, as every cubic inch contains about a quarter of a grain of arsenic.

"The requisite apparatus is as simple as possible; being a glass tube open at both ends, and about three quarters of an inch in its internal diameter. It is bent into the form of a siphon (*aa*, fig. 1), the shorter leg being about five inches, and the longer about eight inches in length. A stop-cock *b*, ending in a jet of fine bore, passes tightly through a hole made in the axis of a soft and sound cork, which fits air-tight

Fig. 1.

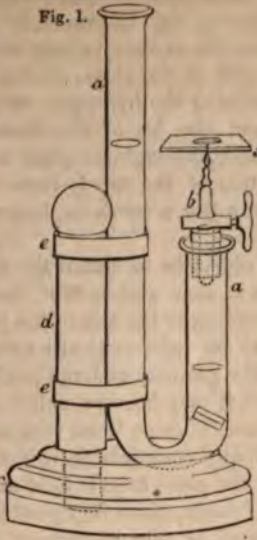
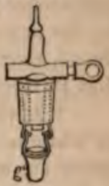


Fig. 2.



the opening of the lower of the tube, and may be ier secured, if requisite, by tle common turpentine lute. ix the apparatus when in in an upright position, a is made in the wooden *c* for the reception of lower part of the pillar *d*, a groove is cut in the top e same block, to receive the of the tube *a a*. Two ic slips *e e*, cut from the of a common bottle of rubber, keep the tube in its place.

The matter to be submitted amination, and supposed to in arsenic, if not in the state, such as pastry, pud- or bread, &c., must be d with two or three fluid ounces of clean water, for a sufficient h of time.

"The mixture so obtained must then be thrown on a filter to separate nore solid parts: thick soup, or the contents of the stomach, may be ed with water and also filtered; but water-gruel, wine, spirits, or any of malt liquor and such like, or tea, coffee, cocoa, &c., can be ated on without any previous process.

"When the apparatus is to be used, a bit of glass rod, about an inch is to be dropped into the shorter leg, and this is to be followed by a of clean sheet zinc, about an inch and a half long and half an inch , bent double, so that it will run down the tube till it is stopped by iece of glass rod first put in. The stop-cock and jet are now to be ted, and the handle is to be turned so as to leave the cock open. fluid to be examined, having been previously mixed with from a hm and a half to three drachms of dilute sulphuric acid (1 acid and ter), is to be poured into the long leg, till it stands in the short about a quarter of an inch below the bottom of the cork. Bub- of gas will soon be seen to rise from the zinc, which are pure ogen if no arsenic be present; but, if the liquor holds arsenic in any in solution, the gas will be arsenuretted hydrogen. The first por- are to be allowed to escape, in order that they may carry with them mall quantity of common air left in the apparatus; after which the is to be closed, and the gas will be found to accumulate in the shorter driving the fluid up the longer one, till the liquor has descended in hort leg below the piece of zinc, when all further production of gas cease. There is thus obtained a portion of gas subject to the pres- of a column of fluid of from seven to eight inches high: when, there- the *stop-cock is opened*, the gas will be propelled with some force *gh the jet, and, on igniting* it as it issues (which must be done quickly by an assistant), and then holding horizontally a piece of crown or window-glass (*f*, fig. 1) over it, in such a manner as to retard slightly the combustion, the arsenic (if any be present) will be found deposited in the metallic state on the glass; the oxygen of the atmosphere being employed in oxydizing the hydrogen only during the process. If no arsenic be present, then the jet of the flame as it issues has a very different appearance; and, although the glass becomes dulled in the first instance by the deposition of the newly-formed water, yet such is the heat produced, that in a few seconds it becomes perfectly clear, and frequently flies to pieces.

"If the object be to obtain the arsenic in the form of arsenious acid, or white arsenic, then a glass tube, from a quarter to half an inch in diameter (or according to the size of the jet of flame), and eight or ten inches in length, is to be held vertically over the burning jet of gas, in such a manner that the gas may undergo perfect combustion, and that the arsenic combined with it may become sufficiently oxydized; the tube will thus, with proper care, become lined with arsenious acid in proportion to the quantity originally contained in the mixture.

"When the glass tube is held at an angle of about forty-five degrees over the jet of flame, three very good indications of the presence of arsenic may be obtained at one operation; viz., metallic arsenic will be found deposited in the tube at the part nearest where the flame impinges,—white arsenic or arsenious acid at a short distance from it,—and the garlic smell can be readily detected at either end of the tube in which the experiment has been made.

"As the gas produced during the operation is consumed, the acid mixture falls into the short limb of the tube, and is thus again brought into contact with the zinc, in consequence of which a fresh supply is soon obtained. This gas, if submitted to either of the processes before described, will give fresh indications of the presence of the arsenic which the mixture may have originally contained; and it will be easily perceived that the process may be repeated as often as may be required, at the will of the operator, till no further proofs can be obtained.

"When certain mixed or compound liquors are operated on in this apparatus, a great quantity of froth is thrown up into the tube, which may cause a little embarrassment by choking the jet. I have found this effect to take place most with the contents of the stomach, with wine, porter, tea, coffee, or soup, and, indeed, with all mucilaginous and albuminous mixtures. The means I adopt to prevent this effect from taking place, or, at least, for checking it in a great measure, is to grease or oil the interior of the short limb of the apparatus before introducing the substance to be examined, or to put a few drops of alcohol or sweet-oil on its surface previously to introducing the stop-cock and its appendages. I have, however, found, if the tube be ever so full of froth in the first instance, that, in an hour or two, if left to itself, the bubbles burst, and the interior of the tube becomes clear without at all affecting the results.

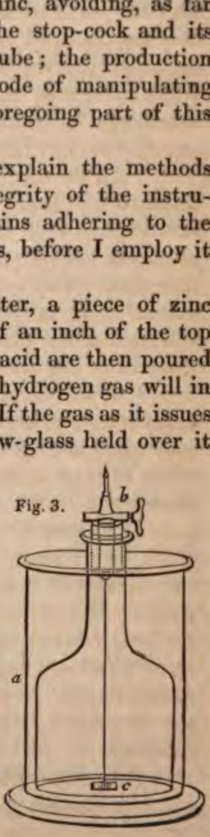
"In cases where only a small quantity of the matter to be examined can be obtained, I have found a great convenience in using the small glass bucket (*g*, fig. 2). Under such circumstances, the bent glass tube be filled up to within an inch of the short end with common water, to allow room for the glass bucket, which must be attached to the , &c. by means of a little platina wire; a bit or two of zinc is to be ped into the bucket, with a small portion of the matter to be nined, and three or four drops of diluted sulphuric acid (acid 2, r 14); and the whole is then to be introduced into the mouth of the t limb of the tube. The production of gas under this arrangement is h slower, and, of course, requires more time to fill the tube, than in former case; but the mode of operating is precisely the same. ed, it is of great advantage, when the quantity of arsenic present is minute, not to allow the hydrogen to be evolved too quickly, in r to give it time to take up the arsenic.

"A slender glass funnel will be found of service when as much as a -spoonful, or even a tea-spoonful, of matter can be obtained for nination. In this case, the tube is to be partly filled with common r, leaving a sufficient space for the substance to be examined; a of zinc is to be suspended from the cork by a thread or wire, so as ang in the axis of the tube; and the fluid to be operated on, having iously been mixed with dilute sulphuric acid, is then to be poured ugh the funnel carefully, so as to surround the zinc, avoiding, as far ossible, to mix it with the water below, and the stop-cock and its ndages are to be replaced in the mouth of the tube; the production e gas then goes on as before stated, and the mode of manipulating it is exactly the same as described in the foregoing part of this r.

"It will be necessary for me, in this place, to explain the methods ploy after each operation, to determine the integrity of the instru- t, so as to satisfy myself that no arsenic remains adhering to the e of the tube, or to the cork and its appendages, before I employ it nother operation.

"After washing the apparatus with clean water, a piece of zinc be dropped in, and the tube filled to within half an inch of the top e short limb; two drachms of diluted sulphuric acid are then poured nd the stop-cock and cork secured in its place; hydrogen gas will in case, as before, be liberated, and fill the tube. If the gas as it issues the jet be then inflamed, and a piece of window-glass held over it fore described, and any arsenic remains, it be rendered evident by being deposited on lass; if so, this operation must be repeated he glass remains perfectly clean, after having exposed to the action of the gas.

Fig. 3.



When I have had an opportunity of working so large a quantity of mixture as from two our pints (imperial measure), I then have oyed the instrument (fig. 3), which is, indeed, a slight modification of one of the instan- ous light apparatuses, now so well known used for obtaining fire by the aid of a m of hydrogen gas thrown on spongy plati- *It will, therefore,* be of importance only for me to describe the alteration which I make when I employ it for the purpose of detecting arsenic. In the first place, I must observe, that the outer vessel *a*, which I use, holds full four pints, and that the jet of the stop-cock is vertical, and its orifice is twice or three times larger than in the instrument as generally made for sale, and also that there is a thread or wire attached to the cork of the stop-cock *b*, for suspending a piece of zinc *c*, within the bell-glass.

"With an instrument of this description I have operated on one grain of arsenic in twenty-eight thousand grains of water (or four imperial pints), and have obtained, therefrom, upwards of one hundred distinct metallic arsenical crusts.

"Similar results have been obtained with perfect success from three pints of very thick soup, the same quantity of port wine, porter, gruel, tea, coffee, &c. &c.

"It must, however, be understood, that the process was allowed to proceed but slowly, and that it required several days before the mixture used ceased to give indication of the presence of arsenic, and also, a much larger portion of zinc and sulphuric acid was employed from time to time, than when working with the small bent tube apparatus, in consequence of the large quantity of matter operated on under this arrangement.

"With the small apparatus, I have obtained distinct metallic crusts, when operating on so small a quantity as one drop of Fowler's solution of arsenic, which only contains 1-120th part of a grain.

"The presence of arsenic in artificial orpiment and realgar, in Scheele's green, and in the sulphuret of antimony, may be readily shown, by this process, when not more than half a grain of any of those compounds is employed.

"In conclusion, I beg to remark, that although the instruments I have now finished describing, are the form I prefer to all that I have employed, yet it must be perfectly evident to any one, that many very simple arrangements might be contrived. Indeed, I may say unequivocally, that there is no town or village in which sulphuric acid and zinc can be obtained, but every house would furnish to the ingenious experimentalist ample means for his purpose; for, a two-ounce phial, with a cork and piece of tobacco-pipe, or a bladder, with the same arrangement fixed to its mouth, might, in cases of extreme necessity, be employed with success, as I have repeatedly done for this purpose.

"The only ambiguity that can possibly arise in the mode of operating above described, arises from the circumstance, that some samples of the zinc of commerce themselves contain arsenic; and such, when acted on by dilute sulphuric acid give out arsenuretted hydrogen. It is, therefore, necessary for the operator to be certain of the purity of the zinc which he employs, and this is easily done by putting a bit of it into the apparatus, with only some dilute sulphuric acid; the gas thus obtained is to be set fire to as it issues from the jet; and if no metallic film is deposited on the bit of flat glass, and no white sublimate within the open tube, the zinc may be regarded as in a fit state for use."

During the Bristol Session of the British Association, the subject of *arsenical* poisons was brought before the Chemical Section by Mr. W. Herapath, of that city. The elaborate paper read by this gentleman was remarkable for the display of information, acute perception, and decisive experiment. As Mr. Herapath was acquainted with the process of Mr. Marsh, he describes it to be "one of the most elegant that can be conceived; at the same time that it is one of the most sensitive." When we consider the time, the place, the speaker, and the audience he was addressing, it is scarcely possible to imagine a higher or more gratifying eulogium. Many of Mr. Herapath's own remarks were too valuable not to be preserved. Among others he observed that:—

Arsenical poisons are obtained with so much facility, and their operation is so deadly, that they are the principal means resorted to by secret poisoners. It becomes, therefore, essential to the safety of the community, that every new fact relating to their administration, operation, or detection, should be made known. Few, if any well-authenticated cases have been published in which death was occasioned by realgar, or red arsenic, but the Burdock case was one of this kind. It will, perhaps, be remembered that the victim, Mrs. Smith, had been buried fourteen months; that upon exhumation orpiment was found in the stomach, and the body was partly converted into *adipocere**. In prosecuting his experiments in this case, he conceived the idea of identifying the poison found with that sold to the witness Evans, by Hobbs, the druggist, by means of an impurity he discovered in the poison of the stomach. With this view he purchased some out of the same box, and requested that it might be of the same kind as that sold the prisoner's agent. It then transpired that the box contained three different substances mixed together: white, yellow, and red arsenic; the two former in small lumps, the latter in powder; and that it was the powder of realgar only which had been administered, although it was undoubtedly found as yellow orpiment in the exhumed body. In tracing the possibility of change, he found that two agents,—sulphuretted hydrogen and ammonia, would either of them, convert realgar into orpiment. Now, as it was well known, that both of these gases were evolved during putrid decomposition, there could be no difficulty in accounting for the change of colour. But to place the matter beyond all doubt, he made a direct experiment by poisoning an animal with realgar, and found that after putrefaction it became changed, as in the case of Mrs. Smith. It would, perhaps, be recollected, that the conviction of the prisoner was mainly owing to the evidence of a little girl, who deposed that she saw Mrs. Burdock put a powder into some gruel, and afterwards administer it to Mrs. Smith. At the time considerable doubt was entertained of the truth of her evidence, from its being invariably precise, even to a word; and also from the difficulty of believing that any person would be found so fool-hardy as to mix and administer poison before a child, and that child a stranger. But what he had stated, proved to demonstration that her evidence was correct, for she said the gruel given "was of a nasty *red* colour;" a colour she could not have had an idea of, unless she had seen it, as nothing had transpired of red arsenic; and had she invented a tale to account for the appearance of the body, or had she spoken from what she had heard from others, she would have deposed to its being of a yellow colour.

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* *Adipocere:* animal matter converted spontaneously into a substance considerably *resembling spermaceti.*

From what had occurred, therefore, it was clear, that the realgar of the shops would cause death. That half an ounce, given at twice (by the prisoner's confession), was sufficient for that purpose. That realgar became orpiment during putrefaction. That realgar, like arsenious acid, had a tendency to control putrefaction, and convert bodies into *adipocere*. During the experiments upon this case, he found that the microscopic system of testing, which was first introduced by Dr. Wollaston, and which he (Mr. H.) constantly followed, could be made to improve the very beautiful reducing process proposed by Dr. Christison; and also furnished an excellent method of proving to the jury the presence of arsenic. He would suppose that the whole of the organic matter had been decomposed, as he did it, in boiling nitro-muriatic acid; that potash had been added in excess to prevent the injurious effects of mineral acid upon sulphuretted hydrogen; and that then a slight excess of acetic acid had been poured in, and the sulphuret of arsenic precipitated; if this were reduced to the metallic state in Berzelius's tube, and then oxidized, as recommended by Christison, it was in the state in which, in subsequent experiments, his modification of Dr. Wollaston's practice was beneficial. Instead of putting the few drops of solution of arsenious acid, thus obtained, into test-tubes to apply the re-agents, he used a china tablet, and having applied a drop of the solution, and then a little ammoniacal sulphate of copper, the green of Scheele became evident, from the contrast of colour with the white plate; but even that might be improved by guiding the coloured drop by means of a glass rod down upon a piece of white blotting-paper, previously placed on a flat chalk-stone, which, by absorbing the solution, removed any excess of the blue re-agent, which was always liable to overpower the colour of Scheele's green, while it left the latter on the paper; when dried it could be introduced, as he usually did it, into a sealed tube, which could be marked with a diamond in the hand-writing of the experimenter, ready for identification before a jury. He felt satisfied that 1-10,000th of a grain of arsenious acid might be rendered evident by this means. The other two re-agents, ammoniacal nitrate of silver and sulphuretted hydrogen, could be applied upon the plate in the same way, and when dried could be similarly enclosed. In all cases where a highly oxygenating process was followed,—for instance, where the mixture was boiled in nitro-muriatic acid, or where deflagration with nitre was practised,—the arsenical compound was converted into arsenic acid, and in passing sulphuretted hydrogen (after the usual precautions), the first portion of the gas was decomposed by giving hydrogen to the oxygen of the arsenic acid; consequently sulphur fell mixed with sulphuret of arsenic, but so extremely light that it took some hours to deposit; after which the mixed deposit could be collected together, and, upon reducing it to metallic arsenic, the sulphur would be separated: for, from being more volatile, it was found above the crust of metal, and in the oxidizing operation it formed sulphurous acid, and disappeared while the arsenious acid condensed.

It sometimes happened that arsenic was contained in substances which prevented the ordinary processes from being followed; for instance, in the case of Sophia Edney, who was convicted at the March assizes, at Taunton, of poisoning her husband. He had found about one-eighth of a grain in the duodenum (the contents of the stomach had been thrown away by the surgeon who examined the body, under the belief that an ulcer found in the stomach was sufficient to account for death): the only other matters brought to him for examination were a few grains of bacon fat scraped from the bottom of a frying-pan. In the fat he could find no arsenic, and the potato being an *amylaceous*[*] substance it was in vain to try the usual re-agents, or to make a filtered solution. He therefore projected it into melted nitre; when it was deflagrated, diluted acetic acid was added until the carbonate of potash, proceeding from the deflagration, was supersaturated. Sulphuretted hydrogen turned it yellow, and upon deposition and subsequent treatment in the way he had alluded to before, he obtained enough to take to a jury specimens of the reduced metal of arsenious acid; Scheele's green, arsenite of silver, and orpiment, were made from it, although he felt satisfied that the reduced arsenic was not more than 1-100th of a grain. It had been said by the dying man that his wife had fried potatoes in this pan for him, and he had not been well since. The prisoner's witness proved that the pan had subsequently been wiped and used to fry bacon, which had been eaten with impunity by two persons, exclusive of the prisoner, who had herself "eaten a bit as big as a nut;" yet, enough had been left adhering to the pan to prove her guilt, which her confession ultimately acknowledged.

Although nitre afforded an excellent means of removing all organic matter, and thus leaving the operator free from all embarrassment, yet it could not be depended on for a quantitative analysis, as a certain proportion is volatilized during the process; this loss might be reduced by putting a little nitre in the solution before evaporating to dryness.

The recent plan of discovering arsenious acid, by converting it into arsenuretted hydrogen, and deposing the arsenical crust during its combustion, is the most elegant that could be conceived: at the same time that it is the most sensitive; but it would require a few modifications to make it the best for exhibition to a jury. First, it was essential that the zinc used to procure hydrogen should have been treated by the experimenter in the same way without arsenic; otherwise the counsel would embarrass the witness by asking if he was certain that arsenic was not contained in the zinc; and next, the metallic crust should be so received as to be kept from atmospheric air, otherwise it would lose its lustre by passing into the "fly-powder" of the Germans. He had found it best to proceed thus:—instead of a plate of glass to cool the flame and receive the crust, he used one of mica, with three drops of water in separate places on one of its surfaces; if the flame was allowed to play under one of those drops, the evaporation of the water kept the part cool, and the crust was thicker, while the risk of fracture was avoided; then by inverting the plate and holding the drops in succession some little height over the flame, they became solutions of arsenious acid, and could be tested with three re-agents as before stated; and if it was necessary to make a quantitative experiment, the products of the flame could be condensed in a large globe, the arsenious acid dissolved and precipitated by sulphuretted hydrogen. The part of the plate of mica, containing the crust, should then be cut off, and introduced into glass tubes hermetically sealed up, like the slips of blotting-paper, containing the coloured results of the re-agents.

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[*] *Amylaceous*, highly pulverized, starch-like.

We think it scarcely possible, certainly not necessary for any practical purpose, that the processes of examination which have been detailed can ever be improved upon. Detection, or the most perfectly satisfactory proof of the non-existence of the poison, in all cases of inquiry, is now certain. Furnished with this unerring test, it may be said that in a country in which the inquest of the coroner is established, there can be no *secret* poisoning. And it may added, that if the knowledge of this test be universally disseminated there will probably be no attempts to commit this crime.

But valuable as this certain detection of the fact when committed is, the prevention of the mischief must be admitted to be still more so. The few precautions which our retail druggists make use of in their sale of poisons, are, no doubt, useful as far as they go, but they are notorious for their inefficiency. The following suggestions of MM. Chevallier and Boys de Loury, which these gentlemen lately submitted to the French Minister of Justice, with the humane intention of rendering the crime of poisoning less frequent, will, therefore, certainly deserve the attention of the learned corporations in this country, whose duty it is to watch over the interests of this branch of public safety; and we are confident that many of the individuals who dispense these deadly articles* to their fellow-creatures will adopt some of the suggestions, without waiting for the compulsory statute of the Hall or College.

M. Chevallier and his colleague are of opinion that poisoning would be rendered less frequent if it were required, by authority, that all poisonous substances should have either taste or colour communicated to them, except in the few cases where it might absolutely destroy their use in the arts.

That the white arsenic intended for steeping of corn, should be mixed with aloes in powder, in the proportion of ten parts aloes to ninety arsenic.

That the arsenious acid used for external applications, by veterinary surgeons and farriers, and by others in their treatment of the itch, should be prepared in the same manner.

That the arsenious acid intended to poison rats, mice, &c., should be mixed with Prussian blue or soluble indigo, in the proportion of ninety parts acid to ten colouring-matter.

That the metallic arsenic in powder, sold for the destruction of flies, should be mixed with a tenth of its weight of soluble blue.

If these precautions were universally adopted, it is highly probable that these gentlemen are correct in thinking, "that in numerous cases taste imparted to food in which poisonous substances had been mingled, would be sufficient to warn the intended victims, and save them from the danger to which they were exposed; and that in other cases, colour so given might have a similar salutary effect."

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* We are sure they will be appreciated *by, at least,* one in the trade—we mean the *cautious* druggist of Barnstaple, who will not suffer them on his premises, and who has inscribed on his house in large characters, "NO OXALIC ACID SOLD HERE."

QUESTIONS FOR SOLUTION RELATING TO METEOROLOGY, HYDROGRAPHY, AND THE ART OF NAVIGATION.

By M. Arago.

[Continued from p. 333, vol. I.]

The springs of Aix, in Provence, have suggested to me a plan of experiment, of which I think it proper to insert a notice, as very probably the physical conditions on which it is founded may be met with in other places.

The town of Aix, in Provence, possesses baths of thermal water, known under the name of the *Baths of Sextius*. They are surrounded by an edifice, the building of which was completed in 1705. The spring was formerly so copious, that in the last two months of that same year, it was amply sufficient for the service of upwards of 1000 bathers. It fully supplied nine jets of a fountain, and nine baths. From the year 1707 the water began to be less plentiful, and in a few months was so much diminished, that the establishment was wholly abandoned.

Other warm springs exist in the town,—at the Cours, in the Garden of the Jacobins, at the Monastery of St. Bartholomew, at La Triperie, Grioulet, the Hotel de la Selle d'Or, the Hotel des Princes, &c., and at the bottom of certain wells, such as that belonging to Sieur Boufillon (at the corner of the Rue des Marchands), and some tanners' pits. These different springs diminished like that of Sextius, and even more rapidly. Many of them, and, among others, those of the Jacobins, of St. Bartholomew, La Triperie, and Grioulet, dried up entirely.

During the period that this diminution of many of the springs of Aix, and the entire destruction of some of them, was going on, individuals began to turn to their private advantage some very copious springs, which they discovered by digging to a small depth in properties situated a little distance from the town, in the territory of Barret. The idea that these new waters were precisely the former waters of the town soon occurred to the minds of many persons; but the impossibility of decisively proving that such was the fact, for a long time prevented the authorities from interfering. At last, in 1721, during the dreadful plague that prevailed in Provence, Dr. Chicoineau of Montpellier, having thought it expedient to order baths for the persons detained in quarantine, Vauvenargues, the commandant of Aix, came to the following resolution: "As the warm baths of the town of Aix appear to us necessary to wash and purify the convalescent patients in quarantine, and the said baths have not sufficient water for this purpose, on account of the quantity that has been withdrawn from the source by various proprietors near it, we order, for the good of the service, that steps be immediately taken to prevent this," &c. &c. In virtue of this order, the consuls caused the holes dug in the district of Barret to be filled up, and, *in twenty-two days after this operation*, the waters of the Baths of Sextius were augmented three-fourths, and many springs which had become entirely dry, that of Grioulet for example, *again began to flow*.

In May 1722, Vauvenargues having been superseded, the dispossessed proprietors pierced, under ground, the works which had been constructed the year before, and immediately the warm springs of the town were seen to diminish, and even entirely dry up.

In July 1722 the breaches were filled up by the vigilance of the authorities, and the inhabitants of Aix saw the waters reappear. Things continued in this state for five years; but in 1727 the tenants of the mills of Barret clandestinely made a new opening in the dam constructed in 1722. The knowledge of this misdeed was only acquired by a falling off in the quantity of water. In order to terminate this obstinate contest between private interest and the general benefit, the town passed an act defining the property, and caused a stone pyramid to be erected upon it, in 1729.

To these details, which we have entered into in order to establish the fact, that the waters of the pyramid of Barret feed the warm springs of the town of Aix, we shall add, that M. Dauphin, locksmith, assured M. Robert, a physician of Marseilles, in 1812, that he witnessed an experiment which places the matter beyond a doubt: he stated, that lime having been mixed with the water in the basin of the pyramid, the springs of Cours and of Mennes became milky.

Under the pyramid of Barret, the basin which the water occupies is also of stone; it is about 11½ feet long and upwards of 6½ feet broad. In June 1812, M. Robert sent down two men to ascertain the temperature of the water; they found it 62·6° Fahr. At the same period, the baths of Sextius were at 84·2° Fahr.

It appears, therefore, established, that the cold waters of Barret become, at least *the greater part*, the warm waters of Aix, by traversing the short space which separates these two points,—that is to say, a horizontal distance, estimated in the official memoirs from which we have given an extract, at about a thousand geometrical paces*.

It will be observed that we have employed the words *the greater part*, and they, in fact, indicate precisely the question which remains to be answered. If it could be proved that all the warm water of the baths of Sextius originated from the cold water of the basin of Barret,—that the phenomenon does not consist merely of an intermixture which may take place near the surface, between the water of Barret and that of an ordinary thermal spring nearer Aix,—and that in its passage the fluid does not become chemically charged with any foreign substance,—the theory of thermal springs would have made a decided step in its progress. Every one would then be satisfied of their similarity to the sources of Artesian wells, the high temperature of which is evidently owing to the great depth from which they issue.

Without pretending to devise the best means of investigation which the survey of the places might suggest, I conceive that if permission were obtained to withdraw the waters of Barret, for a few days only, the principal question would be solved. From the time that the intermediate thermal spring between Barret and Aix should begin to flow to Sextius alone, there would be, simultaneously, a considerable diminution of the quantity of water, and a considerable increase in the temperature of the baths. A comparative chemical analysis of the respective waters, if performed with that scrupulous accuracy of which we have now many examples, would be very interesting. Nor should it be forgotten to repeat the experiment mentioned by the locksmith Dauphin, by employing lime, or bran, or some tinctorial matter, were it only for determining the velocity of the fluid in the subterranean passages which it traverses, in passing from Barret to Sextius.

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* Nearly an English mile.

The temporary turning-off of the waters of Barret, is the most decisive mode of obtaining the solution of that very ancient problem of physical geography to which thermal springs have given rise; but should this deviation be impossible, there still seems to be a method of attaining the object. The waters of Sextius are said to diminish in dry, and to increase in rainy, weather. It is very improbable that the increase and decrease should follow exactly and simultaneously the same relations in the cold, and nearly superficial, waters of Barret, and in those of the thermal spring nearer the town. If a mixture of these waters does take place, we ought, therefore, to expect, that great variations of temperature would be observed at Sextius.

It may be seen, by this single instance, how much the authorities have erred in suppressing the office of Inspector of Thermal Waters, under the idea that nothing in that department remains to be discovered. I now add, in conclusion, that the data on which my plan of experiment is founded, have been derived from a manuscript memoir presented fifteen years ago to the Academy by M. Robert, which has not, in my opinion, met with the attention which it deserves.

Mean Height of the Barometer.—A few years ago a positive denial would have been given to the assertion, that there is any permanent difference between the barometrical heights corresponding to different regions of the globe at the level of the sea. At present such differences are regarded as not only possible, but even probable. The officers of the *Bonite* ought therefore to preserve their barometers with the most scrupulous care in such excellent order, that all their observations, made in every port, may be compared. Notice should never fail to be made of the exact height of the *cistern* of the barometer above the level of the sea.

Of the Influence of different Winds on the Heights of the Barometer.—As soon after the memorable discovery of Torricelli as meteorologists directed their attention to the observation of the barometer, they perceived that, *in general*, certain winds produced a rapid ascent of the mercurial column, while the opposite ones produced a contrary effect, in a manner as equally decided. The difficulty was, to determine the *numerical value* of these influences. It was necessary, in order to eliminate entirely all transient and accidental influences, and to obtain the true measure of permanent causes, to operate upon great numbers; it was necessary to obtain long series of good observations made in the same locality; it was necessary to group the winds according to their precise directions; and, finally, to separate effects purely thermometrical.

Burckhardt undertook this labour, availing himself of twenty-seven years of observations which Messier had made at Paris, from 1773 to

1801. If we designate by the letter H. the mean height of the barometer at Paris, that is to say, the height determined by the average of all the observations, the means corresponding to the different winds, according to Burckhardt's calculations, will be as follows:—

		mm.	Eng. In.			mm.	Eng. In.
South . .	H. *minus*	3, 1	(1·23)	North . .	H. *plus*	2, 0	(0·78)
South-west .	—	2, 9	(1·11)	North-east .	+	2, 6	(1·03)
West . .	—	0, 4	(0·16)	East . . .	+	1, 1	(0·39)
North-west .	*plus*	1, 3	(0·51)	South-east .	+	0, 8	(0·31)

It will be seen, from the mere inspection of this table, that the direction of the wind occasions a variation in the state of the barometer at Paris of 3^{mm}, 1 (1·23 Eng. in.) above the mean, and of 2^{mm}, 6 (1·03 Eng. in.) below it, forming a total variation of 5^{mm}, 7 (2·26 Eng. in.); and that the opposite winds, combined two by two, give a mean height which, in extreme cases, scarcely differs by half a millimetre (0·19 Eng. in.) from the mean of all the observations.

M. Bouvard has presented to the Academy the results of an investigation analogous to that of Burckhardt; it is founded on the observations of the barometer made at the Observatory of Paris from 1816 to 1831, and leads, in general, to the same conclusions. By assigning to the letter H. the signification which we gave it in the preceding table, we shall have the following barometrical heights, corresponding to the different directions of the winds:—

		mm.	Eng. In.	Obs.			mm.	Eng. In.	Obs.
South .	H. *minus*	3, 7	(1·46) .	(2944)	North . .	H. *plus*	3, 2	(1·27) .	(2140)
South-west	—	3, 0	(1·18) .	(2847)	North-east	+	3, 2	(1·27) .	(1390)
West . .	—	0, 8	(0·31) .	(3402)	East . .	+	1, 7	(0·67) .	(1248)
North-west	*plus*	2, 0	(0·78) .	(1533)	South-east	*minus*	1, 7	(0·67) .	(890)

The daily observations at nine o'clock in the morning, at mid-day, and at three in the afternoon, have all concurred in the formation of these numbers. Almost exactly the same results will be obtained by employing only the maxima heights of nine o'clock, and the minima heights of three o'clock.

In this instance, as well as in the table of Burckhardt, half the sums of the heights corresponding to the opposite winds are nearly equal to H., that is to say, to the total mean. The highest mean effect of the wind is 6^{mm}, 9 (2·73 Eng. in.), which surpasses the result afforded by the observations of Messier by 1^{m}, 2 (0·47 Eng. in.)

Both these tables tend to establish a fact with which meteorologists cannot be too strongly impressed,—that in order to obtain in our climates the mean height of the barometer, it is indispensable to admit into the calculation an equal number of observations corresponding to winds of opposite directions.

The tables which we have just transcribed, suggest many scientific questions; they lead us to inquire in what manner this influence of winds on the atmospheric pressure, varies with the position of places, with their greater or less distance from the sea, with their latitude, &c. In the mean time, till data sufficiently numerous be obtained to enable us to attempt the solution of these various meteorological problems, I shall here present to the reader the results of two series of very accurate observations, which were communicated to the Academy by MM. Schuster and Gambart.

The first were made at the School of Artillery and Engineers at Metz, the others at the Observatory of Marseilles.

Observations at Metz, (Nine Years.)

		mm.	Eng. In.			mm.	Eng. In.
South . .	H. *minus*	2, 4	(0·95)	North . . .	H. *plus*	2, 4	(0·95)
South-west .	—	2, 1	(0·83)	North-east .	+	2, 1	(0·83)
West . .	—	0, 6	(0·23)	East . . .	+	1, 0	(0·39)
North-west .	*plus*	0, 3	(0·11)	South-east .	*minus*	0, 8	(0·31)

The difference between the extremes is sensibly less than in the observations at Paris. At the same time. it would be premature to draw general conclusions from this fact, which may perhaps be purely accidental.

The following seems more decisive:—

Observations at Marseilles, (Five Years.)

		mm.	Eng. In.			mm.	Eng. In.
South . .	H. *plus*	0, 0	(0·00)	North . .	H. —	—	—
South-west .	+	0, 7	(0·27)	North-east .	—	—	—
West . .	*minus*	0, 5	(0·19)	East . . .	*plus*	0, 2	(0·07)
North-west .	—	0, 9	(0·35)	South-east .	+	0, 5	(0·19)

Although this table is incomplete, and founded on observations of only five years' continuance, and although the north and north-east winds are entirely omitted, there results from it no less important a consequence than this,—that if the direction of the winds exercises, at Marseilles, any influence on barometrical heights, that influence is very slight, and ought not always, in the case of winds of similar denominations, to have the same sign as in the north of France. Thus, while at Paris the south-west wind depresses the barometer considerably below the mean, its influence at Marseilles is *positive;* on the other hand, the north-west wind, which causes a considerable rise in the barometer at Paris, is that which produces the lowest depression at Marseilles.

When observations such as these have been made at many different places, they will probably place meteorologists in a condition to explain a phenomenon which has hitherto baffled all their efforts.

Of the Diurnal Variations of the Barometer.—Numerous memoirs have been published on the *diurnal variation of the barometer.* This phenomenon has been studied from the equator to the regions in the vicinity of the pole,—at the level of the sea,—on the immense plateaus of America,—on the insulated summits of the highest mountains, and the cause, notwithstanding, remains in obscurity. It is still necessary, therefore, to multiply observations on the subject. In our climates, the vicinity of the sea appears to manifest itself by a sensible diminution in the extent of the diurnal oscillation; does the same thing take place between the tropics?

Observations on Rain.—Navigators occasionally speak of rains which fall on their vessels while traversing the equinoctial regions, in terms which would lead us to suppose that it rains much more abundantly at sea than on land. But the truth still remains in the domain of mere conjecture; so seldom has the trouble been taken to procure exact measurements. These measurements, however, are by no means difficult. *Captain Tuckey, for example,* made many during his unfortunate expedition to the river Zaïre, or Congo. We know that the *Bonite* will be provided with a small udometer (*rain-gauge*.) It seems, therefore, expedient to recommend the commander to cause it to be placed on the stern of the vessel, in such a situation that it can neither receive the rain collected by the sails, nor that which falls from the cordage.

Navigators would add greatly to the interest of these observations, if they would observe at the same time the temperature of the rain, and the height from which it falls.

In order to obtain the temperature of rain with some degree of accuracy, it is necessary that the mass of the water should be considerable, relatively to the size of the vessel which contains it. A metal udometer will not answer for this purpose. It would be infinitely preferable to take a large funnel of some light stuff, very close in its texture, and to receive the water which runs from the bottom in a glass, whose sides are thin, and which contains a small thermometer. So much for the temperature. The elevation of the clouds in which the rain is formed cannot be détermined but during the time of a storm; then, the number of seconds which elapse between the appearance of the flash and the arrival of the sound, multiplied by 1142—the velocity with which sound is propagated—gives the length of the hypothenuse of a right-angled triangle, whose vertical side is precisely the height required. This height may be calculated, if, by means of a reflecting instrument, we obtain the angle formed with the horizon by a line which, passing from the eye of the observer, terminates in that quarter of the cloud where the lightning first showed itself.

Let us suppose, for an instant, that there falls on the vessel rain whose temperature is below that which the clouds should possess, according to their height, and the known rate of the decrease of atmospheric heat; every one will understand the consequences which such a result would produce in meteorology.

Let us suppose, on the other hand, that during a day of hail (for it hails in the open sea), the same system of observations had proved that hail-stones were formed in a region where the atmospheric temperature was higher than the point at which water congeals,—science would thus be enriched with a valuable result, which every future theory of hail must necessarily account for.

We could adduce many other considerations to demonstrate the utility of the observations we have proposed; but the two preceding must suffice.

RAIN IN A PERFECTLY CLEAR SKY.—There are some extraordinary phenomena, concerning which science possesses but few observations; and for the reason, that those who have had the opportunity of witnessing them avoid describing them, from an apprehension that they might be regarded as undiscerning visionaries. In the number of these phenomena we may rank certain rains of the equinoctial regions.

Sometimes *it rains* between the tropics when the atmosphere is perfectly pure, and the sky of the most beautiful azure! The drops are not very numerous, but they are larger than the greatest rain-drops in our climates. The fact is certain; we have the evidence of M. von Humboldt that he has observed the occurrence in the interior of continents, and Captain Beechey states that he has witnessed it in the open sea. With regard to the circumstances on which such a singular precipitation of water depends we are entirely ignorant. In Europe we sometimes see during the day, in cold and perfectly clear weather, small crystals of ice falling slowly from the air, their size increasing with every particle of humidity they congeal in their passage. Does not this approximation put us in the way of obtaining the desired explanation? Have not the large rain-drops been at first, in the higher regions of the atmosphere, small particles of ice excessively cold; then have they not become, as they descended, large ice-flakes by means of accumulation; and when lower still, have they not melted into drops of water? It will be readily understood that the only object with which these conjectures are brought forward in this place is, to show in what point of view the phenomenon may be studied, and to stimulate our young travellers, in particular, to observe carefully if, during these singular rains, the region of the sky from which they fall presents any traces of halo. If such traces are perceived, however slight they may be, the existence of crystals of ice in the higher regions of the air would be demonstrated.

In the present day there is scarcely any country where meteorologists are not to be found, but it must be confessed that their observations are usually made at hours selected without proper discernment, and with instruments either inaccurate in themselves, or improperly placed. It does not now appear difficult to deduce the mean temperature of the day from observations made at any hour; thus a meteorological table, whatever may be the hours of observation in it, may be possessed of value, by the mere condition that the instruments employed will admit of comparison with a standard barometer and thermometer.

We think it proper to recommend these comparisons to the officers of the *Bonite*. Wherever they can be effected, local meteorological observations will be of value. A collection from the newspapers of countries will often supply what would otherwise be obtained with difficulty.

MAGNETISM.

DIURNAL VARIATIONS OF THE DECLINATION (VARIATION)*.—Of late years science has been enriched with a considerable number of observations on the diurnal variations of the magnetic needle; but the greater part of these observations have been made either in islands, or on the *western sides* of continents. Corresponding observations made on the *eastern sides* would at present be very useful. They would serve, in fact, to submit to an almost decisive test the greater part of the explanations of this mysterious phenomenon which have been promulgated.

The route prescribed for the expedition does not allow us to suppose that the *Bonite* can harbour or even remain some time at points situated between the terrestrial and the magnetic equators, such as Pernambuco, Payta, Cape Comorin, and the Pelew Islands. Had it been otherwise, we should have particularly recommended the erection of M.

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* The *declination* of the magnetic needle is popularly, but improperly, called the *variation*, in Great Britain.

Gambey's beautiful instrument, in a firm position, at a distance from every ferruginous mass, and that the oscillations of the needle should have been attended to with the most scrupulous care*.

INCLINATION (or *Dip*).—In general it will be attended with little advantage to bestow much care on observing the diurnal variations of the horizontal magnetic needle in places where the expedition is not stationary for a whole week. It is different, however, with the other magnetic elements. Wherever the *Bonite* stops, though it be only for a few hours, it would be desirable to measure, if possible, the declination, the inclination, and the intensity.

In the attempts to reconcile observations on the inclination made at remote periods, in different regions of the earth, in the neighbourhood of the magnetic equator, it was ascertained, some years ago, that this equator is advancing progressively and entirely from the east to the west. At present it is supposed that this movement is accompanied with a change of form. The study of lines of equal inclination, regarded under the same point of view, will not be less interesting; when all these lines shall have been traced upon the charts, it will be curious to follow them with the eye, in all their displacements and changes of curvature; important truths may emanate from such an examination. It will now be understood why we require as many measurements of inclination as can be collected.

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* At all events we shall here present the problem, which observations made at the points mentioned would serve to solve. *In the northern hemisphere*, the end of an horizontal magnetic needle, which *points towards the north*, moves from the *east* to the *west* from 8¼ o'clock A.M. to 1¼ in the afternoon, and from *west* to *east* from 1¼ P.M. to the following morning. Our hemisphere cannot be peculiar in this respect; the same effect produced on the north end here, must be produced on the south end to the south of the equator. Thus, *in the southern hemisphere*, the end of an horizontal magnetic needle which *points towards the south*, will move from *east* to *west* from 8¼ A.M. till 1¼ in the afternoon, and from *west* to *east* from 1¼ P.M. till the morning of the next day. Further, observation corroborates the supposition. Now let us compare the simultaneous movements of the two needles, when comparing the same end, namely, *that which points towards the* NORTH. *In the southern hemisphere, the end pointing towards the* SOUTH moves from *east* to *west* from 8¼ A.M. to 1¼ P.M.,—therefore the north end of the same needle makes a contrary movement. Thus, finally, *in the souhern hemisphere, the end pointing towards the* NORTH moves from *west* to *east* from 8¼ A.M. till 1¼ P.M., which is precisely opposite to the movement made by *the north* end, at the same hours, in our *hemisphere*.

Let us suppose that an observer, starting from Paris, advances towards the equator. So long as he continues in our hemisphere, the *north end* of his needle will make a movement every morning *towards the west;* in the other hemisphere, the *north end* of the same needle will move every morning *towards the east.* It is impossible that this change from a *western* movement to an *eastern* one can take place in a sudden manner. There is, necessarily, between the zone where the first of these movements is observed and that where the second takes place, a line, where, in the morning, the needle will neither move to the *east* nor to the *west*,—that is to say, it will remain stationary.

Such a line must exist; but where is it to be found? Is it a curve of equal intensity, or the magnetic, or the terrestrial equator?

Researches, continued *during many months*, in the places situated between the terrestrial and magnetic equators, such as Pernambuco, Payta, Conception, the Pelew Islands, &c., would certainly lead to the desired solution. But *many months* of assiduous observation would be requisite; for, notwithstanding the skill of the observer, the short stay of Captain Duperrey at Conception and Payta, made at the request of the Academy, have left some doubts on the subject.

The question has been often agitated, whether, in a determinate place, the dipping-needle would mark exactly the same degree at the surface of the ground, at a great height in the air, and at a great depth in a mine. The absence of uniformity in the chemical composition of the earth, renders the solution of this problem very difficult. Observations of the measurements made in a balloon are not sufficiently exact. When the *physicien* takes his station on a mountain, he is exposed to local attraction; ferruginous masses may there greatly alter the position of the needle, and nothing be present to point out the effect. The same uncertainty attends observations made in the galleries of mines. Not that it is absolutely impossible to determine the influence of accidental circumstances in each place; but then for this purpose it is necessary to have instruments of the most perfect kind;—to be able to go from the station chosen, in all directions, and to great distances; and, finally,—to repeat the experiments a greater number of times than a traveller has generally an opportunity of doing. But, however this may be, observations of this kind are worthy of attention. Their mass will, perhaps, one day lead to some general result.

With regard to the declination, its immense utility is so well known to navigators, that any recommendation on the subject would be superfluous.

Observations on Intensity.—Observations on the intensity are not of earlier date than the travels of Entrecasteaux and M. von Humboldt, and yet they have already thrown a strong light on the complicated, and at the same time highly interesting, subject of terrestrial magnetism. Observations of this nature ought, in the highest degree, to attract the attention of the officers of the *Bonite*, for at present the theorist is arrested at every step by the want of exact measurements.

The aërial excursions of MM. Biot and Gay Lussac, undertaken some time since under the auspices of the Academy, were in a great measure designed for the examination of the following important question: Has the magnetic force, which, on the surface of the earth, directs the magnetic needle towards the north, exactly the same intensity at every height to which it may be elevated?

The observations of our two fellow-members, those of M. von Humboldt in mountainous countries, and the still older observations of Saussure, all seem to concur in showing, that at the greatest heights which man has yet reached there is no appreciable decrease in magnetic force.

This conclusion has recently been disputed. It has been remarked, that, in the ascent of M. Gay Lussac, for example, the thermometer which indicated 87·8° Fahr. on the ground at the time of departure, sunk as low as 15·8° Fahr. in the region of the atmosphere where the needle was made to oscillate a second time. And as it is now satisfactorily proved that the same needle, occupying the same place, and under the influence of the same force, will oscillate so much the more quickly, as the temperature is diminished, it becomes necessary, on account of the state of the thermometer, that a certain reduction be made in the intensity indicated by the higher observations, in order that those made in the *balloon and on the earth* may be comparable. Without this correction, the needle would appear equally attracted above and below; therefore, in spite of appearances, there was a real decrease. This diminution of the magnetic force with the elevation seems likewise to result from the observations made in 1829, on the summit of Mount Elbrouz (in the Caucasus), by M. Kupffer. In this case an exact account was taken of the effects of temperature, and yet various irregularities in the series of inclinations threw some doubt on the result.

We conceive, therefore, that comparisons of the magnetic intensity, at bases and at summits of mountains, ought to be particularly recommended to the officers of the *Bonite*. Mowna-Roa, in the Sandwich Islands, seems to be well adapted for the purpose. Observations may likewise be made on the Tacora, if the expedition stop for a few days at Arica.

LUMINOUS METEORS.

ON LIGHTNING.—M. Fusinieri has been lately studying the effects of lightning under an entirely new point of view.

According to this *physicien*, the electrical sparks issuing from ordinary machines, which we see as they traverse the air, contain brass in a state of fusion, and incandescent molecules of zinc, when they emanate from a brass conductor; if the sparks issue from a ball of silver they contain impalpable particles of that metal. In the same way, a globe of gold gives rise to sparks, which contain, during their passage through the air, melted gold, &c. &c.

The molecules in the centre only of all these sparks are melted; on their exterior surfaces the metallic particles undergo a greater or less degree of combustion, in consequence of their contact with the oxygen of the atmosphere.

When a spark issuing from a globe of gold passes through a silver plate, even of considerable thickness, there is seen on the two surfaces of the plate, at the points where the electric spark entered and emerged, a circular stratum of gold, the thickness of which must be very inconsiderable, since, if left to itself, it volatilizes, and disappears entirely in a short time. According to M. Fusinieri, these two metallic spots are formed by the fused gold which the electric spark contained. The deposit on the first face is nothing extraordinary; but, by adopting the explanation of the Italian *physicien* for the spot on the opposite surface, we are obliged to admit, that the gold disseminated through the spark has passed, at least in part, along with it through the whole thickness of the silver plate! It is unnecessary to add, that a spark issuing from a ball of copper gives rise to similar phenomena.

A spark emanating from one metal, and passing through another, does not only lose a portion of the molecules with which it was at first charged, but it acquires new molecules at the expense of the metal traversed. M. Fusinieri even asserts, that, at each passage of the spark, reciprocal changes are produced between the two metals present; that when the spark, for example, leaves silver to pass to copper, it not only *transports* a portion of the first metal to the second, but that it likewise *transports* copper to the silver! I will insist no longer, however, on these phenomena; I have cited them here only with a view to show that the sparks of our ordinary machines contain ponderable matter.

M. Fusinieri affirms that similar matter exists in lightning, and that in this case also it is in states of minute division, of ignition, and of combustion. According to him, this transported matter is the true cause of the transient smells which thunder occasions when it falls, and also of the pulverulent deposits which remain round fractures through which the electrical matter has forced a passage. In these deposits, which have been too much neglected hitherto by observers, M. Fusinieri has detected metallic iron, iron in different degrees of oxidation, and sulphur. The ferruginous spots left on the walls of houses may be found, when strictly examined, to arise from the iron with which the lightning was charged, derived from that which occurs in every building; but what is to be said regarding the sulphureous spots on these same walls, and especially the ferruginous marks which are found on trees struck with lightning in the open fields? M. Fusinieri conceives himself authorized to infer from these experiments, that the atmosphere contains, at every height, or at least as far as the region of stormy clouds, iron, sulphur, and other substances, on the nature of which chemical analysis has been hitherto silent; that the electrical spark is charged with them, and that it carries them to the surface of the earth, where they form attenuated deposits round the points struck with the lightning.

This new method of regarding electrical phenomena, assuredly deserves to be followed up with that accuracy which is suited to the present state of science. Every one who witnesses a stroke of lightning, would perform a very useful service by carefully collecting the black or coloured matter which the electrical fluid seems to deposit, at all those stages of its progress where it undergoes sudden changes of velocity. A careful chemical analysis of these deposits may lead to unexpected discoveries of high importance.

Falling Stars.—Ever since observations were made with accuracy on falling stars, it has been evident how greatly these long-despised phenomena,—these pretended atmospheric meteors,—these so-called trains of inflamed hydrogen gas, are deserving of attentive examination. Their parallax has already placed them much higher than the sensible limits given to our atmosphere by received theories*. During the inquiry concerning the apparent direction in which these falling stars *ordinarily move*, it was ascertained that even though they are inflamed in our atmosphere, they do not originate in it, but that they enter it from without. The direction *most common to them, seems diametrically to be opposed to that of the earth in its orbit!*

It is desirable that this result should be established by the investigation of a numerous series of observations. We have therefore requested that officers on watch on board the *Bonite* may note, during the whole of the voyage, the hour at which a falling star may appear, its probable angular height above the horizon, and particularly, the *direction of its motion.*

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* Comparative observations made in 1823 at Breslau, Dresden, Leipsic, Brieg, and Gleiwitz, by Professor Brandes and many of his pupils, have assigned no less than 500 English miles as the height of certain falling stars.

The apparent speed of these meteors is found sometimes to be 36 miles per second. This is *nearly double that of the* earth's motion round the sun. Even although we were inclined to regard half of this apparent velocity as an illusion arising from the effect of the earth's movement in its orbit, there would still remain 18 miles per second as the real velocity of the star, a degree of rapidity which exceeds that of all the superior planets, except the earth.

By referring these meteors to the principal stars of the constellations which they traverse, the different questions which we have indicated may be resolved at a glance. Here, then, is a subject of research which requires no trouble. It may suffice to attach our young countrymen to the subject, to remark how interesting it would be to establish the fact of the earth being a planet, from proofs derived from such phenomena as falling stars, the inconstancy of which has become proverbial. We might add, if it were necessary, that it is scarcely possible at present to imagine any other mode of explaining the astonishing appearance of bolides (*fiery meteors*) observed in America on the night of the 12th and 13th of November, 1833, than by supposing that, besides the large planets, there move round the sun myriads of small bodies which are not visible but when they penetrate into our atmosphere, and there become inflamed; that these asteröids (to adopt the name which Herschel long since applied to Ceres, Pallas, Juno, and Vesta,) move in some way or other in groups; that others, however, are isolated; and that the assiduous observation of these falling stars will be, at all times, the only means of enlightening us in regard to these curious phenomena.

We have just mentioned the appearance of falling stars noticed in America in 1833. These meteors succeeded each other so quickly, that they could not be counted; but a moderate calculation makes their number amount to hundreds of thousands*. They were seen along the eastern side of America from the Gulf of Mexico to Halifax, from nine o'clock in the evening to sunrise, and even, in some places, in daylight, at eight o'clock in the morning. *All these meteors issued from the same point of the sky*, situate near γ Leonis; and that, notwithstanding the altering position of this star in consequence of the diurnal movement of the sphere. This, then, is assuredly a very remarkable fact, and we shall cite another which is not less so. The shower of falling stars in 1833 took place, as we have already said, on the night of the 12th and 13th of November. In 1799, a similar shower was observed in America, by M. von Humboldt; in Greenland, by the Moravian fraternity; and in Germany, by various persons. The date is in the night between the 11th and the 12th of November.

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* The stars were so numerous, and appeared in so many different regions of the sky at once, that in the attempt to reckon them, nothing more than a very rough approximation could be expected. An observer at Boston compared them, when at the maximum, to half the number of flakes seen in the air during an ordinary fall of snow. At a time the phenomenon was considerably on the decrease, he counted 650 stars in 15 minutes, although he circumscribed his observations to a zone, which did not include a tenth part of the visible horizon. This number, in his opinion, was not more than two-thirds of the whole; thus there must have been 866, and in the whole of the visible hemisphere, 8660. This last number would give 34,640 stars per hour. As the phenomenon lasted seven hours, the number that appeared at Boston must have exceeded 240,000; for it must not be forgotten that the data on which these calculations are founded, were not collected till the phenomenon was considerably on the decline.

In 1832 Europe, Arabia, &c., were witnesses of the same phenomenon, but on a smaller scale. The date of this appearance is again the night between the 12th and the 13th of November.

This near approach to identity in the dates, authorizes us the more to invite our young navigators to watch attentively whatever may appear in the sky from the 10th to the 15th of November, since observers who were favoured with a clear atmosphere, and who watched for the phenomenon last year (1834), saw manifest traces of it on the 12th and 13th of that month*.

The Zodiacal Light.—The zodiacal light, although known for nearly two centuries, still presents a problem which has not been solved in a satisfactory manner. The study of this phenomenon is chiefly reserved, by the very nature of things, to observers placed in the equinoctial regions. They alone can decide whether Dominico Cassini had sufficiently guarded against the causes of error to which an observer is exposed in our variable climates, and whether he had sufficiently taken into account the purity of the air, when he announced in his work that the zodiacal light is constantly brighter in the evening than in the morning; that in the course of a few days its length may vary from 60° to 100°; that these variations are connected with the appearance of solar spots, in such a manner, for instance, that there must have been an absolute relation, and not merely a fortuitous coincidence, between the weakness of the zodiacal light in 1688, and the absence of every kind of spot, luminous or otherwise, on the solar disc in that same year.

It appears to us, therefore, that the Academy ought to request the officers of the *Bonite*, during the whole time they remain between the tropics, and when the moon does not enlighten the horizon, to be on the watch, either after sunset or before sunrising, and take note of the constellations which the zodiacal light traverses, of the star nearest its point, and of the angular breadth of the phenomenon near the horizon, at a determined height. It is almost superfluous to add, that an account must be kept of the hours when the observations were made. The investigation of the results may be delayed without any inconvenience till the period of returning home.

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* Since my report was read to the Academy, M. Berard, one of the best informed officers in the French navy, has had the kindness to address to me the following extract from the journal of the brig *Loiret*, of which he was the commander:—

"On the 13th November 1831, at 4 o'clock A. M., the sky being perfectly clear, with abundance of dew, we saw a considerable number of falling stars, and luminous meteors of a large size, For upwards of three hours there could not, on an average, be fewer than two every minute. One of these meteors, which appeared in the zenith, left an enormous train in a direction from east to west, and formed a broad luminous band (equal to half the diameter of the moon), in which many of the colours of the rainbow were very distinctly seen. Its track was visible for more than six minutes. We were then on the coast of Spain, near Carthagena; the thermometer in the air, 62·6° Fahr.; barometer, 30·3 in.; temperature of the sea, 65·2°."

On the 13th November 1835, a large and brilliant meteor observed by M. Millet Daubenton, fell near Belley, in the department of Ain, and burned a barn. On the same night a falling star, more *brilliant than Jupiter*, was observed at Lille by M. Delezenne. It left behind on its route, a train of sparks in every respect resembling those produced by a squib.

Thus, all these facts tend more and more to confirm the notion, that there exists a zone composed of millions of small bodies, whose orbits meet the plane of the ecliptic near the point which the earth occupies every year, from the 11th to the 13th of November. It is a new planetary world just beginning to be revealed to us.

It is doubtless unnecessary for me to say, how important it would be at the present moment to inquire whether other *trains of asteroïds* do not meet the ecliptic in points different to that in which the earth is placed about the 13th of November. This investigation would require to be made, for example, from the 20th to the 24th of April; for in 1803 (I believe it was on the 22nd of April), there were seen in Virginia and Massachusetts, from one o'clock till three in the morning, falling stars in all directions, and in such numbers, that they might be compared to a shower of rockets.

Messier relates that, on the 17th June 1777 about noon, he saw during five minutes a prodigious number of black globules passing across the sun. Might not these globules be asteroïds likewise?

We are not ignorant, and we have already hinted it, that some very able minds consider these statements of Dominico Cassini as little deserving of confidence. They are unwilling to admit that sensible physical changes could operate simultaneously through such an immense extent as the zodiacal light embraces. In their opinion, these variations in intensity and length, noticed by this great astronomer, were not real, and nothing further than intermissions of the atmospheric transparency are required to account for them.

It would not now, perhaps, be impossible to prove, by comparing the observations of Fatio with those of Cassini, that atmospheric variations are insufficient to explain the phenomena described by the Parisian astronomer. With respect to the objection derived from the immensity of the space in which the physical changes must operate, it has lost all its force since we have witnessed similar phenomena exhibited by Halley's comet.

Our young countrymen ought therefore zealously to devote themselves to such observations as we have pointed out. The question is important, and no person can hitherto flatter himself with having definitively solved it.

AURORA BOREALIS.—It is now well ascertained that there are as many displays of polar aurora in the southern hemisphere as in the arctic regions. Everything leads us to think, that the appearances of the southern aurora, and of that which we witness in Europe, follow the same laws. This, however, is mere conjecture. If a southern aurora be seen by the officers of the *Bonite* in the form of an arch, it will be important to notify exactly the bearings of the intersections of this arch with the horizon, and, if these cannot be obtained, the *bearing of the most elevated point*. In Europe, the most elevated point always appears to be situated in the magnetic meridian of the place where the observer is stationed.

It has been proved by numerous researches undertaken at Paris, that all kinds of aurora borealis, even such as do not appear above our horizon, and the existence of which, consequently, we can learn only from the reports of observers in the polar regions, alter decidedly the declination of the magnetic needle, as well as its inclination and intensity. Who, then, can presume to argue from the great distance of an aurora australis, that it never disturbs the magnetism of our hemisphere? Every case in which the attention of our travellers shall make a correct memorandum of these phenomena, may at length throw some light on the question. Such arrangements have been made, that magnetic observations will be made at Paris during the whole time of the circumnavigation of the *Bonite*, at periods so near each other, and in such a manner, that no perturbation can take place unobserved.

The Rainbow.—The explanation of the rainbow may be regarded as one of Descartes's most beautiful discoveries; but, still, even after the developements which Newton has furnished, it is yet incomplete. When we look attentively at this magnificent phenomenon, we perceive under the red of the interior arch several series of green and purple, forming narrow contiguous arches, well defined, and perfectly concentric to the principal arch. Of these *supplementary* arcs (for that is the name given to them,) the theory of Descartes and Newton takes no notice, and indeed it cannot even be applied to them.

The supplementary arcs appear to be an effect of *luminous interference*. These interferences cannot be produced but by drops of water of a certain smallness. It is necessary also, for otherwise the phenomenon would have no brilliancy, that, besides this condition of magnitude, the drops, or at least the greater part of them, should be almost mathematically equal in their dimensions. If, therefore, the rainbows of equinoctial regions are never attended with supplementary arcs, it would be a proof that the drops of water which there issue from the clouds are of larger size, and more unequal dimensions, than in our climates. In our ignorance of the causes of rain, this fact would by no means be void of interest.

When the sun is low, the upper portion of the rainbow is, on the contrary, very much elevated. It is towards this culminating region that the supplementary arcs show themselves in greatest splendour. Descending from this, their colours become rapidly fainter. In the lower regions, near the horizon, and even considerably above it, no traces of them are ever seen, at least in Europe.

It follows, therefore, that rain-drops, during their vertical descent, lose the property which they at first possess; that they have no longer the conditions necessary for efficient *interference*, and that they increase in size.

Is it not curious, it may be asked in passing, to find in an optical phenomenon, in a peculiarity of the rainbow, a proof that in Europe the quantity of rain must be so much the less the higher we place the vessel in which it is to be received*!

The increase in the size of the drops, it can scarcely be doubted, is owing to a precipitation of humidity on their surface; this will be in proportion to the atmospheric strata through which they pass in their descent from the cold region of their origin; and which strata are warmer and warmer, as they approach the earth. It is then almost certain that, if supplementary rainbows are formed in equinoctial regions, as in Europe, they never reach the horizon; but a comparison of the angle of the height at which they cease to be seen with the angle of disappearance noticed in our climates, seems to offer a means of obtaining some meteorological results, which can be obtained by no other method at present known.

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* In the Observatory at Paris, there are two vessels in which rain-water is collected; *one of them is on the terrace, the other in the court, 92 English feet lower than the* first. In the course of a year the reservoir in the court received eight-hundredths *more* water that that placed on the terrace.

Halos.—In high latitudes, off Cape Horn, for example, the sun and the moon often appear surrounded by two luminous circles, which meteorologists call *halos*. The radius of the smallest of these circles is about 22°,—that of the larger is almost exactly 46°. The first of these angular dimensions is within a little the minimum deviation which light undergoes while traversing a glass prism of 60°; the other would be given by two prisms of 60°, or by a single prism of 90°.

It seems, therefore, natural to seek for the cause of halos, as Mariotte has done, in the rays refracted by floating crystals of snow, which, as every one knows, usually present angles of 60° and 90°.

This theory, besides, has been rendered still more probable since the power has been acquired of distinguishing refracted from reflected light by means of chromatic polarization. It is, in fact, the colours of the first kind (refracted light) which produce the polarized rays of the halo. What, then, still remains to be known regarding this phenomenon? It is the following:—According to theory, the horizontal diameter of a halo and the vertical diameter ought to have the same angular dimension; but we are assured that these diameters are sometimes strikingly unequal. Actual measurement alone can establish this fact; for if it has happened that a judgment of the inequality in question has been formed by the naked eye only, there are not wanting sufficient causes of illusion to explain how the most experienced *physicien* might be deceived. The reflecting circles of Borda are admirably adapted to the measurement of the angular distances at sea. We may, therefore, without hesitation, recommend to the officers of the *Bonite* to apply the excellent instruments with which all of them are provided to determine the dimensions of all the halos *that may appear to them to be elliptical.* They will themselves perceive that the inner edge of the halo,—the only one which is distinctly defined,—is much better adapted for observation than the outer one; but they must never, with regard to the sun, neglect to indicate whether they take the centre or the edge as the term of comparison. We likewise regard it as indispensable, that, in each direction, the two rays diametrically opposite should be measured,—for certain observers have mentioned circular halos, in which, if they are to be believed, the sun did not occupy the centre of the curve.

WINDS.

Trade-Winds.—Perhaps it will excite surprise to see the trade-winds announced as still affording a subject of important investigation; but it must be remarked, that the practice of navigators has often confined them to meagre notices; with such science cannot be satisfied. Thus it is not true, whatever may have been alleged, that to the north of the equator these winds constantly blow from the *north-east;* and that to the south of it they blow uniformly from the *south-east.* The phenomena are not the same in the two hemispheres. In each plane, moreover, they change with the seasons. Daily observations of the true direction, and, as far as practicable, of the strength of the eastern winds which prevail in equatorial regions, would therefore be a useful acquisition to meteorology.

The vicinity of continents, their western sides especially, modifies the trade-winds in their strength and direction. It sometimes even happens that they are displaced by a *west* wind. Wherever this change of the wind occurs, it is desirable to note the time, the bearing of the neighbouring coasts, their distance, and, where practicable, their general aspect. In order to show the utility of this last recommendation, it will be enough to say, that a sandy country, for example, will have a much speedier and more active influence, than one covered with forests, or any other kind of vegetation.

The sea which washes the western shore of Mexico, from Panama to the peninsula of California, between 8° and 22° north latitude, will afford an opportunity to the officers of the *Bonite* of observing a complete inversion of the trade-winds. They will find, as stated by Captain Basil Hall, a nearly permanent west wind, in a situation where it might be expected the east wind of the equinoctial regions would prevail. In these offings it would be curious to ascertain how far from the shore this anomaly extends,—at what degree of longitude the trade-wind resumes, so to speak, its dominion.

According to the most generally adopted explanation of the trade-winds, there ought to exist, between the tropics, a *more elevated wind*, constantly blowing in an opposite direction to the one on the surface of the earth. Numerous proofs are already collected of the existence of this counter-current. Vigilant observations of elevated clouds, particularly of those called *cirro-cumulus*, would certainly furnish indications of great value to meteorology.

Finally, the periods, strength, and extent of the monsoons, form subjects of study, in 'which, notwithstanding a vast amount has been collected, there is still something to glean.

(To be continued.)

TRANSACTIONS OF THE ROYAL ASTRONOMICAL SOCIETY.

In continuation of the plan we proposed (see p. 209) to adopt with respect to the labours of different learned societies, of giving, as occasion may arise, a more or less brief analysis of the papers published in the Transactions of those bodies, we now proceed to lay before our readers an account of the contents of the concluding section of the ninth volume of those of the Royal Astronomical Society. In our next number we propose to give a similar analysis of the Transactions of the Royal Society of Edinburgh.

This part contains ten papers, besides the Report of the Council, the Anniversary Address, and other miscellaneous information, chiefly relating to the management of the Society.

I. *On the Time of Rotation of Jupiter. By* F. G. Airy, Esq., *Astronomer Royal, and President of the Society.*

Kepler inferred, from a presumed connexion between the time of *the rotation and that of its* first satellite, that Jupiter's time of rotation was less than 24 hours. Cassini was led from his own observations in 1664-5 to conclude that it was less than 14 hours. Subsequent observations in 1665 led him to the time of $9^h\ 56^m\ 0^s$. This is the period adopted by Laplace, and all subsequent writers.

In December, 1834, Professor Airy took advantage of one of the two remarkably black and well-defined spots which appeared near the lower belt of the planet, to make a series of observations, with a view to the determination of the period of Jupiter's rotation with greater accuracy. These observations extend over the period from December 16th to March 19th following: and from them, by methods which he details, he finds that the true period corresponding to them is $9^h\ 55^m\ 21{\cdot}3^s$, mean solar time.

This close approach (differing by only 38·7 seconds) to the period determined by Cassini, will seem remarkable enough to those who estimate by numerical differences the degree of approximation, without considering the unit by which those differences are measured: but to those who are accustomed to look upon an inquiry in all its details, and under careful mathematical discipline, it is obvious that this small quantity may be in reality a comparatively great one. It is so in the present case.

The time of the visibility is less than five hours; and the period of observation extending over 225 revolutions, from which Mr. Airy deduced his period, it would make 225 × 38·7 seconds, or $2^h\ 25^m\ 0{\cdot}75^s$, or about half the period of the visibility of the spot itself. The period then assigned by Cassini is, as the author remarks, incompatible with the observations recorded and discussed in his communication.

II. *Continuation of Researches into the Value of Jupiter's Mass. By the same.*

It would be hardly possible to render the objects of this short paper intelligible to those who are unacquainted with the problem itself, and with the previous papers of the author on the subject. We propose, therefore, to enter into some degree of explanation of the general character and the nature of the difficulties encountered in it, in an early number, as well as such historical details as we think may be interesting to our readers. It is sufficient to state here, that Professor Airy finds the whole mass of the Jovial system $\frac{1}{1048{\cdot}8}$ of the sun's mass. The result of observations discussed in his former papers on the subject was $\frac{1}{1048{\cdot}3}$.

III. *On the Position of the Ecliptic, as inferred from Observations with the Cambridge Transit and Mural Circle, made in the year* 1834. *By the same.*

This determination is founded on 138 transits of the sun, in which both the preceding and following limbs were observed, and 143 polar distances, in which both the north and south limbs were observed. Professor Airy believes these determinations, in point of individual accuracy and general excellence, to be equal to any that have been used for the same purpose.

1. The author determines the place of the equinox to be intermediate

between those assigned by Bessel and Pond, but a very little nearer to the former.

2. That the obliquity of the ecliptic assumed in the *Nautical Almanac* (namely, 23° 27′ 39·26″,) is too great by 0·357″.

3. That the sun moves, independently of the effects of perturbations, in a small circle, whose distance from the north pole of the ecliptic is 90° — 0·569″.

This last is a startling conclusion. Mr. Airy is evidently embarrassed by it: but to our minds it appears clear that it must arise from either imperfection of method, or imperfection of instruments. We cannot imagine the circumstances indicated by it to be the case of actual nature.

IV. *Description of the Mural Circle at the Armagh Observatory, and Examination of its Divisions. By* T. R. Robinson, D.D., *&c.*

This paper, which, in reference to the actual construction of mural instruments, consists almost entirely of practical details, is unsusceptible of material abridgement. The author comes to the conclusion that the present method of dividing large circles is insufficient for attaining the degree of accuracy which practical astronomers now require; and that to effect it satisfactorily, they must be *divided on the engine*, as Reichenbach divided his three-feet circles.

V. *Report on the new Standard Scale of this Society [the Royal Astronomical]. Drawn up at the request of the Council, by* Francis Baily, Esq., F.R.S.

Of this elaborate paper we shall give an analysis in a future number.

VI. *Two elementary Solutions of Kepler's Problem by the Angular Calculus, By* William Wallace, A.M., F.R.S.E., *&c., Professor of Mathematics in the University of Edinburgh.*

A planetary orbit being an ellipse, having the sun in one of its foci, Kepler discovered (by a discussion of the observations of Tycho Brahe) that the radius vector of a planet describes equal areas in equal times: but the truth of this law, as the result of any hypothesis respecting the force of gravitation, was first proved by Newton.

The equation to which it leads is

$$z = x - e \sin x;$$

in which z is the mean anomaly, x the true anomaly, and e the eccentricity of the elliptic orbit.

When x is given, z is readily found from a table of sines; but when z is given, and x required, (which is the form in which the problem always presents itself in astronomical research,) it becomes a matter of considerable difficulty, and oftentimes excessively troublesome. Various methods have been proposed,—of course, chiefly tentative, and n[illegible]rily inelegant combinations of geometry and trigonometry.

We shall give the conclusions of Professor Wall[illegible] readers to the work itself for their investigations, as w[illegible] *by which he illustrates them.*

First method.—1. Find x' an approximation to x by this formula.

$$\tan\left(x' - \tfrac{1}{2} z\right) = \frac{1+e}{1-e} \tan \tfrac{1}{2} z.$$

2. Find y such that

$$\tan\left(\tfrac{1}{2} x' - y\right) = \frac{1+e}{1-e} \tan \tfrac{1}{2} x'.$$

3. Find c, the correction of x', so that

$$\sin(y - c) = \frac{\sin y}{\sin x'} \cdot \frac{\sin 1''}{e} (x' - z),$$

expressing $x' - z$ in seconds of a degree.

4. Then the eccentric anomaly, $x = x' + c$.

Second method.—Find x' an approximation to the value of x in the given equation, $z = x - e \sin x$, and form a successive series of angles connected by the following equations; they will be closer and closer approximations to the true value of x at every step.

$$x^{\text{ii}} = z + \frac{e}{\sin 1''} \cdot \sin x'.$$

$$x^{\text{iii}} = z + \frac{e}{\sin 1''} \cdot \sin x''.$$

$$x^{\text{iv}} = z + \frac{e}{\sin 1''} \cdot \sin x'''.$$

. . . .

$$x^{n} = z + \frac{e}{\sin 1''} \cdot \sin x^{n-1}.$$

In these solutions, Professor Wallace suggests, that as in each planet, e, $\frac{e}{\sin 1''}$ and $\frac{1+e}{1-e}$ are constant quantities, they may be conveniently tabulated for each of the several orbits.

VII. *Sixth Catalogue of Double Stars, observed at Slough in the years* 1831 *and* 1832, *with the twenty-feet Reflector; containing the Planes, Descriptions, and measured Angles of Position of* 286 *of those Objects, of which* 105 *have not been previously described. Reduced to the Epoch of* 1830·0. *By* Sir John Frederick William Herschel, K.G.H.

This is a continuation of a former series published in these Transactions. It is arranged in columns of

No. | AR. 1830·0 | N.P.D. 1830·0 | Posit. | Dist. | Magn. | Remks. | Sweep. | Ref.

Of course no particular description can be given. The value of the paper consists in its determination of the position and character of the several objects contained in it. Only two observations occur of general interest beyond that which arises from a determination of the places. One is, that an observation in sweep 425, an observation of η *Coronæ*, was made with personal hazard; and the other that the triple star in sweep 392 appears to have changed since sweep 172 was made, including the *same stars*, they being in the first (172) in a straight line, but in the *latter (392)* no longer so.

VIII. *Some Particulars respecting the principal Instruments at the Royal Observatory at Greenwich, in the time of Dr. Halley. By* S. P. Rigaud, Esq., A.M., F.R.S., *Savilian Professor of Geometry at Oxford.*

The Royal Observatory owes its existence to the indefatigable Sir Jonas Moore. It was placed under the direction of the Ordnance Department, with which Sir Jonas was so intimately connected; the salary of the Astronomer Royal was paid at the Ordnance Office; and all supplies for that institution were always made through the same medium, till very recently, when it was transferred, we think judiciously, to the governance of the Admiralty Board. The Master-General of the Ordnance has seldom been a man of science, or a man of right feeling in respect of science, (the Marquis Townshend is, indeed, a splendid exception, and, in more recent times, Sir James Kempt, during his short occupation of the post, is another:) and hence it has happened, that, for a national establishment, the Royal Observatory has been most inadequately provided for, both in actual payment to its labourers, and in respect to the supply of requisite instruments. The Admiralty Board, besides being the most natural guardian of astronomy, is also better fitted on several accounts, for carrying on the superintendence of the appropriate' business of such an institution, both on account of its interest in the results, and its particular constitution. It is nominally under the direction of men who know little of science, and care for it as little; but generally under the actual direction of one man, chosen because he is acquainted with the details of its business, and, consequently, fully impressed with a sense of the necessity for the results which are only to be obtained from such a source, and, therefore, deeply interested in its being supplied with all the requisite means for carrying it on successfully. The Board of Visiters, also, exercise a salutary influence, both upon the Government and the Astronomer Royal of the time being.

None of these advantages were attached to its connexion with the Board of Ordnance; and yet, by degrees, after the most painful and almost degrading solicitations, the Observatory became supplied with a tolerably good set of instruments, of almost every kind. Still the British Observatory was generally behind most of the continental national ones; and, in the *essential* instruments, was often behind more than one corporate, or even private, establishment devoted to the same objects. It is only very recently that science has been admitted to be of *national* importance: and, even now, it is little more than half-admitted. This, however, is not the place for amplifying the discussion of the causes of this unhappy trait in the mania of the day.

This very interesting paper of Professor Rigaud contains an account of the instruments which Bradley, the successor of Halley, and third Astronomer Royal, found in the institution upon his appointment to the office. Our readers will recollect that the learned Radcliffe observer published the miscellaneous works of Dr. Bradley, in three quarto volumes, a few years ago; and in these he gave a general account of those instruments at the period of 1742. No opportunity of printing the several distinct notices found amongst the papers of Bradley, relating to the tim
Halley, presented itself till the present. Mr. Baily's Life of Flam

contains ample accounts of the state of the Observatory during the lifetime of the first Astronomer Royal; and Professor Rigaud has, from the papers above mentioned, and from other sources, filled up the chasm, so far as Halley's occupancy of the office is concerned.

The instruments of the Observatory in Flamsteed's time were his own private property; and the jealousy subsisting between Halley and his predecessor, was an unconquerable difficulty in the way of any fair arrangement between the widow of Flamsteed and the man whom, from her husband, she had learned to hate most cordially. Halley was in his sixty-fourth year when he was appointed; and, upon his appointment, found the Observatory "wholly unprovided of instruments; and, indeed, of everything else that was moveable."

The descriptions themselves cannot be well abridged. The paper itself is, however, full of interest, not only to the practical astronomer, but also to every one who takes the slightest interest in the history of science.

IX. *Observations on Halley's Comet. By Captain* W. H. Smyth, R. N., F.R.S., *&c.*

Captain Smyth appears to be the first who saw the Comet, on the present appearance of that remarkable body, with the naked eye. This was on the 19th of October, 1835, in crossing the great quadrangle of Trinity College, Cambridge, and it even appeared with "a much finer effect" than through the telescope on the summit of the principal gate-tower of the College which he had just left.

These observations will be embodied in a paper which we are preparing, and which will appear in an early number, containing a condensed account of the appearances of the Comet on its several apparitions, so far as they have been recorded. It is sufficient to state here that they were made at Bedford, with the usual instruments and precautions, and, in addition thereto, with an excellent annular micrometer, which was lent to Captain Smyth by Mr. Francis Baily for the purpose.

The analytical difficulty of the problem, resulting from the very rapid motion of the comet, is very neatly solved by the "worthy assistant-secretary of the Society, Mr. Epps;" and "the beautiful cometary ephemeris furnished to the astronomical world by Lieutenant Stratford has afforded the means of computing, to satisfactory exactness, the hourly velocities, equating for second and third differences."

X. *Astronomical Observations.*

These are of a miscellaneous kind, both as to objects and observers; and very judiciously thrown together in a tabular form. We shall merely state them generally.

1. Observed transits of the moon, and moon-culminating stars, at the Observatories of Greenwich, Cambridge, and Edinburgh.
2. Observed occultations of the fixed stars and planets; by Messrs. Rothman, Henderson, Snow, Fisher, Sheepshanks, Hartnup, and Wrottesley.
3. Observed eclipses of Jupiter's satellites; by Messrs. Rothman, Henderson, and Sir Everard Home.

4. Observations on the Solar Eclipse, November 30, 1834; by Sir Home, and Mr. Byron Drury, in Jamaica.

5. Observations on the Solar Eclipse, May 15, 1835; by thirty-nine ferent observers.

6. Observations of Halley's Comet. At South Kilworth, by Dr. arson; at Blackheath, by J. Wrottesley, Esq.; and at various places sea, by Captain R. Owen, R. N., and officers of H. M. S. Thunderer.

7. Results of Lunar Observations made at Edinburgh, in the years 34 and 1835. By Professor Henderson, Astronomer Royal for otland.

the address of the Council, or the President's speech on the delivery the medal, though they enter somewhat into the history of the labours the Society, we do not feel ourselves called upon to offer an opinion. very one who looks to the labours of the practical astronomer as bear- g upon the decision of the structure of the universe, will be interested the brief but comprehensive discourse delivered by the President on at occasion, respecting nebulæ and multiple stars: still we cannot per- ade ourselves that so fertile a topic might not have been more effi- ently treated than he has done. We must confess, however, that we finitely prefer the manly and subdued tone of the Astronomer Royal of ngland, to the laboured splendours of the Astronomer Royal of Ireland. Professor Airy sometimes says too little, Professor Hamilton scarcely er fails to make atonement for him by saying a great deal too much: the one does not say all that *might* be said, the other says a great deal ore than a philosopher *ought* to say. We prefer the former error to e latter: the philosopher injures his own respectability, and degrades s science itself, when he becomes a special pleader for Philosophy.

MISCELLANIES.

Biot and Newton.

A MEMOIR on astronomical refraction was lately read at the *Académie des Sciences*, in which the distinguished author, M. Biot, successfully proved that there was one honour, not hitherto supposed to belong to him, yet due to the illustrious Newton, namely, that of having discovered the theory of astronomical refraction. M. Biot having perused with close attention the letters of Newton to Flamsteed, published in the past year by Mr. Baily, found in them the complete series of ideas that Newton had followed out in order to calculate the table of refractions which was subsequently published in his name by Halley, in the *Philosophical Transactions*, 1721, without the slightest reference to the means employed to form it. It is thus certain that Newton was in possession of differential expressions of astronomical refractions similar to those used at the present day, and that he had deduced from them, theoretically, his table for the case of an uniform temperature. M. Biot, in order to exhibit them in a simpler form, went through the calculations of Newton, and by them arrived at the same numerical results as those which are furnished by the methods now in use.

"Thus," remarked M. Biot, "we have yet to add to the vast number of other discoveries made by this great man that of the theory of astronomical refraction. If we reflect that he was obliged, as his correspondence shows, to discover for himself, one after another, all the physical bases, and all the meteorological elements, of this theory, and that at a time when no person but himself suspected that the indications of the barometer and of the thermometer had the slightest relation with these refractions, and that he at length attained, approximatively it is true, but by a method direct, and due to himself alone, the same numerical values which the mathematicians of the following age regarded as one of the grandest results of the improved integral calculus, we shall, without doubt, admit that such an acquisition was one of the finest works of his immense genius, and one which, beyond all others, best demonstrates the sagacity with which he seized all the constituent elements of the complicated phenomena which he submitted to investigation."

That this act of justice to the departed English mathematician should have been neglected by his scientific countrymen is, to say the least, remarkable;—that it should have been rendered by a Frenchman, and by a philosopher so eminently qualified to appreciate the discovery, and that he should have deduced it from a correspondence supposed, by some persons, slightly to tarnish the lustre of a splendid reputation, is one of the most striking and gratifying events in the progress of modern science.

Is there no academy of either nation (for both countries are now equally interested in the commemoration of this glorious event,) patriotic enough to offer the following subject for a prize in painting? BIOT *announcing to the* ACADÉMIE DES SCIENCES, *a century after the death of* NEWTON, *the discovery by the illustrious Englishman of the theory of Astronomical Refraction.*

Apparent Connexion of Aurora Borealis with Rain, Wind, and decreasing Atmospheric Pressure.

AN examination of our Meteorological Journal for October will show that an unusual quantity of rain fell during that month, particularly in the early part of it, and that it was in this wetter period that the auroras were of considerable splendour, and most numerous. The first aurora observed, occurred on September 30th. On the following day there was a decreasing barometric column, a strong south-west wind, and torrents of rain. On October 4th another aurora occurred, and on the next day a decreasing column, and a fall of rain amounting to nearly an inch and a half.

An aurora on the 11th, in the evening, displayed, during four hours, luminous arches, and a vast field of light. Again, on the next day, there was a gradually decreasing column; rain; and then came on those south-west gales which were so severe and fatal on the coasts.

The last observed aurora was visible on the 18th, and attracted universal notice in the metropolis, from the great extent of its deep-red colour*. Rain followed, but the barometric column, in this case, did not sink; on the contrary, it steadily rose, and its ascent exceeded the mean height.

A comparatively dry period succeeded; the atmospheric pressure remained high until the 27th: it then fell with the north-westerly wind which preceded and brought in the late cold weather and deep snow.

Advantage of Chlorine in the Conversion of Iron.

It has recently been discovered at an iron-work in Germany, that by producing a disengagement of chlorine in the finery-furnaces used for converting iron into the second and third qualities, an article may be obtained equal to that of quality No. 1. This new process has been carried into effect in the furnaces on the Lower Rhine, and is said to have completely succeeded.

Mean Level of the Sea.

The level of the sea, to which the barometric measurements of altitude are referred, as to a standard level, is obtained, in the practice of the most eminent French hydrographers, by taking the mean between the average height of two consecutive flood-tides and that of their intermediate ebb; or, reciprocally, the mean between the average height of two consecutive ebb-tides and the height of their intermediate flood.

The Tide, a true Barometer.

In a memoir on the Tides of the Coasts of France M. Daussy states, that he has been induced to believe, from a great number of observations made at Brest, that when the weight of the atmosphere elevates the mercurial column of a barometer ⅛ in., the mean level of the sea, (taken as in the preceding article,) is depressed about fourteen times as much, or 1¾ inches. The proportion of the specific gravity of mercury to sea-water being as 13·3 to 1, M. Daussy concludes that the mean level of the sea may be considered a true barometer, its changes always corresponding with those of the atmospheric pressure.

This remarkable result having been disputed by Mr. Lubbock,—as, in the observations made on the tides at London under his supervision, no indication of this connexion was detected,—M. Daussy felt obliged to ascertain, by experiments made at other points, if the fact observed at Brest was merely local, depending upon the immense basin, so nearly shut in, which spreads itself before the entrance of that port. He took advantage, for this purpose, of some tidal observations, which were ordered to be made by the Government in 1835, on different parts of the French coast, and principally of those at L'Orient, where the localities were favourable to the convenient observation of high and low water. He compared these with the barometric observations made at the same place during the months of August, September, October, November, and December, 1835. The result of this comparison confirmed his previous opinion, of the correspondence between the greatest heights of the mean level of the sea and the lowest of those of the barometer, except that the motion of the mean level is a little more marked at L'Orient than at Brest.

M. Daussy was further convinced that this effect was not owing, as some persons are disposed to believe, to the influence of the wind. "To determine this," he says, "I observed, successively, during each kind of wind, regarding both its force and direction, and examined if the heights given by the mean level did not, in each series, follow an order analogous to that of the barometer; I was soon convinced, that the same law of variation did take place, and in all winds,—that is to say, that the wind remaining constant in direction and force, it was always found that the height of the mean level varied with the effect of atmospheric pressure." The number of these observations was small, and the conclusions drawn from them can apply to the port of L'Orient only: they are the following. First, That light winds have little influence on the height of the mean level, whatever may be their direction. Second, That fresh breezes have still less influence. Third, and lastly, That gales and strong winds from the N. and N.E. depress the mean level about $3\frac{1}{4}$ inches, and that similar winds from the S.W., S., and S.E., elevate it to the same amount.

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* About 8 P.M., police, firemen, and boys, were running in all directions, expecting to find a fire in the next street, or at *farthest, in the next street* but one. The *bustle and perplexity* lasted about an hour.

Since the publication of these latter observations of M. Daussy, Mr. Lubbock has been induced to examine further into this interesting cause of variation in the mean level of the sea, and which had, previous to M. Daussy, escaped attention. He has announced, that Mr. Dession, having, at his request, calculated the heights and times of the tides at Liverpool for the year 1784, and compared the results obtained by him with the barometric heights of the same year made by Mr. Hutchinson, this conclusion is arrived at, namely, that for $\frac{1}{10}$ in. depression of the mercurial column, there is a corresponding elevation of the mean level of the sea, amounting to one inch. With regard to the epochs of the tides, no sensible connexion with the atmospheric pressure was observed. This is a remarkable and satisfactory corroboration of the fact observed by M. Daussy. It may now be desirable to adopt the term, *Standard Level of the Sea,* (or some similar one,) when elevations are referred to the sea-level,—meaning by the new term, the mean level of the sea at a certain constant height of the barometer.

Lifting of the Kremlin Bell.

In the month of July, in the present year, a successful attempt was made to raise the enormous bell which had been so long buried in the earth, in the Kremlin, at Moscow. This bell, one of the wonders of Moscow, was cast, in 1733, at the command of the Empress Anne, by a Russian founder, Michael Motorine. It is, according to Clarke, 21 ft. $4\frac{1}{2}$ in. high; at two feet from the bottom its circumference measures 67 ft. 4 in.; its diameter at that height is consequently about 21 ft. 6 in. Its *thickness,* at the part intended to be *struck by* the hammer, 23 in. The *Russians* estimate the weight at 12,000 *poods,* which is nearly 200 English tons. The reputed elegance of its form, the style of its bas-reliefs, and the richness of its metal, composed of gold, silver, and copper, contributed to make it remarkable as a specimen of the advanced state of the art of casting in Russia, at the epoch of its execution.

M. Montferrand, a gentleman greatly distinguished in Petersburgh by the numerous works he has executed, was intrusted with the direction of the operations. As the bell was lying in a cavity in the ground, and more than thirty feet below the surface, a large excavation was made to clear it. Over this was constructed a strong and lofty scaffold* for the attachment of the blocks, and for the temporary suspension of the bell at a proper height. At half-past five in the morning of the 5th of July last, the authorities of Moscow and a large number of spectators being assembled on the spot, prayers were offered up for the success of the attempt, and the operations commenced on a signal given by M. Montferrand. Six hundred soldiers instantaneously set-to at a large number of capstans. The enormous weight was mastered, and the bell was soon seen to rise slowly in the pit. Forty-two minutes elapsed during its elevation to the necessary height. No accident occurred. The first operation being finished, the next was to build a platform beneath the suspended bell. This was completed in eight hours, and the bell lowered upon it. On the following day it was placed on a sledge, and drawn, by means of an inclined plane, up to the pedestal intended to support it, and there finally left, on the 26th of the same month.

This colossal work of art is, after all, but a mere curiosity. Its use as a bell is impossible, from a fracture, about seven feet high and two feet wide, in the lower part, where, as has been stated, it is 23 in. thick. The cause of this gigantic injury rests entirely upon conjecture.

Thermometer indicating Mean Temperature.

A method of accurately ascertaining the mean temperature of the atmosphere at any given time and place has long been a *desideratum,* not only among meteorologists, but among *phy-*

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* Said to have been fifty feet from bottom to top.

sicians in general; for, independent of other important considerations, it is intimately connected with the determination of the law that regulates the distribution of heat over different climates; and it is only by obtaining the mean temperature of a place at different and distant periods, that the great question can ever be solved, as to whether the temperature of this country, or of the globe in general, is increasing, stationary, or diminishing.

Ordinary thermometers, it is well known, give the temperature of the atmosphere at the moment of observation only; there are others contrived to indicate the maximum and minimum temperatures which may have occurred between any two selected epochs,—but no instrument has yet been constructed which would accurately ascertain, and register, the mean temperature of the periods between two such epochs, however short it might be.

A skilful timepiece-maker of Copenhagen, M. Jules Jurgensen, eminent for the excellence of his chronometers, and for a Treatise on Detached Escapements, has succeeded in an attempt to produce such an instrument. It is, in fact, by means of chronometers that he exhibits the mean temperature desired*.

It is generally known that, to prevent variations of temperature affecting the going of a watch, there is attached to the balance-wheel of good instruments a curved bar, composed of two metals, the unequal dilatation and contraction of which, under the same temperature, shortens and extends the curve, and thus accelerates or retards the motion.

To apply this principle to the purpose of marking mean temperatures, M. Jurgensen first reverses the arrangement of the metals in the curved bar, so that, instead of compensating for any variation of temperature, it magnifies the effect of it; he then increases this sensibility to variation by adding a second arc, and by these means he obtains a variation of 31½ seconds for each degree of temperature. It may now, after a little reflection, be conceived, that if this instrument be compared, at two distant instants, with a chronometer keeping regular time, we could ascertain, by changing the differences of time into degrees, the mean actual temperature of the period. It would be necessary, of course, previously to adjust this instrument to the thermometer, so that the march of the two instruments may be uniform and comparable. In order to render these chrono-thermometers still more useful, M. Jurgensen adds, without much increasing their bulk, a metallic thermometer, which indicates present temperature, and this, by the aid of two slides upon it, is made to give also the maximum and minimum temperatures which may have occurred between any two instants of observation.

This ingenious instrument, therefore, gives, on inspection,—

1. The temperature of the present moment.
2. The maximum temperature of the period between the present moment and that of any other moment previous to it which we have selected.
3. The minimum temperature, and,
4. The mean temperature of the same period.

The principal objection that presents itself to this desirable instrument is the cost, which necessarily occurs in all cases where chronometers are required.

Indelible Writing Ink.

DURING a recent discussion in the *Académie des Sciences* on the merits of some paper prepared to prevent the fraudulent removal of characters written upon it, M. Dulong called to the recollection of the Academy, that a commission appointed by them had demonstrated, that the surest means of rendering written characters indelible, was to use Indian ink dissolved in water, with a slight mixture of some acid, more particularly the hydrochloric.

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* In the Edinburgh Encyclopædia, Art. *Atmospherical Clock*, a machine is stated to have been proposed by Dr. Brewster, which should record any variation of temperature that takes place during a given period, and indicate on the dial-plate the exact average of all the heights of the mercury in the thermometer. The principle is, "that the variations of heat and cold affect the pendulum, which may be either of the tubular or gridiron kind, and which is so constructed as to render sensible, in the motion of the clock, the alternate contractions and dilatations which it undergoes." To numerous inquiries as to the actual existence of this instrument, we have never been able to give a satisfactory answer. A full description was promised to be given in a subsequent part of the Encyclopædia,—but *either this was not done, or it* has eluded *more than one attempt to find it.*

Parisian Mechanics' Institution.

An Association for the instruction of artificers, &c. has been in activity in Paris for about four years, and latterly with remarkable success.

The first idea of it is said to have originated with the late venerated geometrician, Monge. The members are, principally, students of the École *Polytechnique*, and other benevolent and enlightened persons [illegible] classes. The [illegible]

[illegible] ments, and drawing of the human figure and ornaments.

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible] and the prevention, [illegible] disease.

[illegible] neglect of [illegible] geometry in this country we exposed and remarked upon recently*; and, unwilling to be merely querulous complainers, we, at the same time, commenced an attempt to render, as far as may lie in our power, this delightful and extensively useful branch of science better known amongst us.

It would be a waste of time to dwell upon the vast importance of an intimate acquaintance with the means of preserving health, and of avoiding disease, to individuals and to society.

No limit can be assigned to its beneficial consequences,—and yet how deplorably deficient are all our great establishments of individual and professional education, with respect to this valuable species of knowledge! The care of the public health is absolutely abandoned†, until perhaps the nose or the pocket of some influential individual is affected. If he raises the question of a nuisance, how lamentable then is the hostile array of scientific ignorance. On questions much less complicated and obscure than the effects of miasma, vapours, &c. upon the human body, there is not the slightest difficulty, in any case, of obtaining any number and weight of opinions on one side, and balancing these by an equal weight and number on the other! and so unsettled is the general mind on these subjects, that this species of evidence, so morally [illegible], reflects no disgrace upon [illegible].

On the [illegible] visit of a dangerous epidemic, [illegible] absurdities are promulgated and adopted as infallible preventives or cures! No intelligent guide or competent adviser is at hand, and the pestilence may rage, not only without check, but with aggravated mischief, in consequence of the general ignorance. It is well when masses, as well as individuals, profit by adversity and misfortune. The effects of the cholera at Paris were severely felt, and though the attacks of that dreadful visiter no precaution known can evade, yet it is gratifying to see the authorities of Paris, roused by the terrors which it produced, sparing no cost, no labour, to remove causes supposed to be favourable to its extension. The general attention is now sensibly alive in that metropolis to the advantages of cleanliness, drainage, and ventilation, and we have little doubt that this excellent idea of teaching the principles of Hygiène to the workmen who attend this institution suggested itself at the same epoch.

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* Page 306 of the present volume.

† Thus [illegible] several millions of gallons of muriatic acid gas precipitated weekly and uninterruptedly from the summit of a tall chimney, upon a crowded population in a large town. This is now, and has been for [illegible] years, the case at Newcastle; [illegible] the survivors of the immediate neighbourhood, if there are any, it is not intended to continue much longer. A process recently patented by Mr. Maugham, the Chemical Lecturer at the Adelaide-street Gallery, is about to be adopted at the manufactory in question. By this process the gas, immediately on its evolution, is made to enter into a new combination, and, instead of rushing up the chimney to create a large deal of annoyance, and [illegible], it will never enter the chimney at all, but be instantaneously fixed in a saleable product, viz. chlorate of lime used in bleaching.

It is fortunate that, though discoveries in this important science may require the united assistance of several others, and the subtlest investigation of their most intelligent cultivators, yet its practice may often be reduced to measures of mere precaution, and generally to operations of great ease and simplicity. A more popular or interesting subject could scarcely be suggested to the many able lecturers who so frequently address the numerous auditories of every class during a London winter. And let us ask, why should not a science which would so essentially and largely contribute to the happiness of all mankind, be taught from a Chair in every university and public school? What immense fields are there for its exercise in our crowded manufactories, our mines, our prisons, our ships, our over-peopled districts, indeed, in every single dwelling, and in the habits and daily life of every individual!

The Botanical Society of London.

THE friends of botanical science, which we recently mentioned[*] as likely to embody themselves into a society, have finally done so, under the title of THE BOTANICAL SOCIETY OF LONDON. They meet at their rooms, 11, John Street, Adelphi, every alternate Thursday, at 8, P.M. The anniversary of the society is fixed for the 29th of November, that being the birth-day of the celebrated English botanist, Ray. It is with pleasure that we insert the following passage from the prospectus of this association:—"Ladies are eligible as members, it being well known that there are many who have devoted their attention with success to this delightful study, and whose occupations often leave them much leisure for observation and research."

Number of British Species of Plants.

THE number of species of plants found in Great Britain is about 1500. This comprises all those which exist in Lapland and Sweden, with scarcely any exception. It includes about three-fourths of those which grow in Germany, and about two-fifths of those of France; the southern and sea-board provinces of the latter country adding greatly to the variety of its vegetable productions.

The vicinity of London may be considered extremely rich in objects of botanical research; for of the 1500 species of plants belonging to Great Britain, about 1000 may be found within 25 miles of the metropolis. Mr. Irvine has observed 670 of them within two miles of Hampstead, and 900 within the same distance from Croydon. Thus there are great inducements to the inhabitant of this smoky city to issue from its loaded atmosphere, and pursue the delightful and healthy researches of the botanist.—IRVINE, *Meeting of the Botanical Society of London, Nov.* 1836.

Bored Well at Grenelle.

THE well now boring in the slaughter-house (*abattoir*) of Grenelle, near Paris, had in September last been carried down to a depth of 1150 feet. When about 1000 feet deep, the temperature at the bottom of the bore was 72° Fahr.; at the surface of the ground it was 54° only.

Geological Co-operation.

"Is the granite on the right bank of the Elbe, in Saxony, more recent than the chalk?" has long been a fertile source of dispute among German geologists. Dr. Cotta, who had examined the country with MM. Humboldt and Rose, has written largely upon it, but argues that, to arrive at any degree of certainty upon the subject, explorations beneath the surface are desirable. At the late scientific meeting at Jena this proposition met with a favourable reception, and Dr. Cotta has, in consequence, undertaken, by aid of subscriptions, to make the excavations, &c. necessary to ascertain the fact. He invites the geologists of all countries to co-operate in accomplishing the work.

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[*] *See p. 321.*

The objects to be obtained are,—

1. To determine, by excavations and borings in the valley of Polenz, in what manner the granite is superposed upon the *Grès*.

2. To expose to view, on the road from Rotherwalde to Hohnstein, on the right slope of the valley of Polenz, the boundary of the granite and the *grès*, in order that accurate observations may then be made of their reciprocal penetration, if such exists, and of the phenomena of their contact.

3. Finally, to examine the geological circumstances of the jurassic and free-stone *(quader-sandstein)* strata.

Each subscriber of a *conventions-thaler* (about 4*s*. Eng.) is considered as the holder of a share; this will entitle him to a pamphlet, descriptive of the operations and their results, with engravings, and an account of the expenditure.

The cost is thus estimated:—

Object No. 1,	from	£10 to	£30.
„ 2,	„	£ 4 to	£ 6.
„ 3,	„	£20 to	£30.
Printing, engraving, and paper . .		£14 to	£16.
		£48	£82

Many distinguished geologists have become subscribers; among others, MM. Humboldt, Weiss, Leonhard, Naumann, Rose, Noeggerath, &c.

Danger of Calomel. Medico-Botanical Society's subject for Gold Medal.

"AMONGST the chemical preparations that have attained great celebrity is calomel, a medicine which enfeebles all the vital powers, and which may from that circumstance derive its efficacy in subduing active inflammation; but its operation is eminently injurious; and it cannot be sufficiently deplored that it should be rashly and ignorantly employed as a domestic remedy, and even as an ordinary aperient, instead of being reserved solely for those disorders which might seem to require it, under the advice, and by the authority, of a medical practitioner. As it is frequently thus misapplied, and as fashion, which is so often synonymous with folly, has promoted its use even in the tender age of infancy, we cannot be surprised that nervous disorders are common, that bodily vigour and mental energy are impaired, and that cases of insanity have become more numerous. An eminent physician, whose experience in such cases was very extensive, and whose opinions were founded upon accurate observations, assured me that insanity had, in many instances, arisen from the injudicious employment of calomel; and such must naturally be the result, when both the mind and the body are debilitated by factitious means, when the infirmities of old age are prematurely produced, when life becomes languid, and the power no longer exists of enjoying the gifts of Providence, and of sustaining with composure the cares and vexations of our earthly pilgrimage. Under such circumstances, an infirmity may become intolerable, and the mental faculties may be disturbed, if not destroyed; but even when such lamentable consequences do not ensue, a shattered constitution, a sort of nominal existence in a melancholy and miserable state of dejection and debility, with enfeebled nerves and almost exhausted powers, may be more afflicting to the patient than a chronical disorder. It will be said, and I am ready to admit, that a mercurial preparation has not, in an equal quantity, the same action on different individuals, and that some persons are more susceptible than others of its injurious effects; but this circumstance furnishes an additional argument against the unnecessary administration of such a remedy, since its power, in any particular case, can be learned only from experience, and is sometimes found to be greater than was expected or wished by the physician. So extensive is the misuse of mercurial preparations, and so injurious are their ultimate operation, that it has been most properly determined, by the Council of this Society, to offer the gold medal for the best Essay on the question, "*What is the vegetable substance which could be employed with success as a substitute for mercury in the cure of syphilis*, or of diseases of the liver?"

"It would be of extreme importance if

effectual substitutes could be discovered for those medicines which, from their potency, may be dangerous, if not fatal, when they are misapplied, and which, even when they remove a disorder, may produce permanent injury to the patient. That such substitutes may be found in some disorders, was shown in a case which came under my personal observation, of a lady to whom I was related, and who had been accustomed, for the purpose of allaying the pain arising from an internal complaint, to take opium, of which the dose was gradually increased till it amounted to a considerable quantity; but it was last discovered, though it was then too late to remedy the evils which had been thus occasioned, that the same relief was experienced from drinking soda water. Nothing would more contribute to the advancement of medical science, to the honour of the medical profession, and to the benefit of patients, than the cure, by safe and simple means, of difficult or dangerous diseases, instead of employing, as is too frequently done, and even in cases of a different description, substances which are powerful, but poisonous, and therefore pernicious."—EARL STANHOPE, *Address to Medico-Botanical Society*, 1836.

Great Central Heat of the Earth not alarming.

"IT appears, that if there be an increase of temperature in descending, such a fact can result from nothing but a central heat, independent of existing influences.

"The discussion of the evidence of this fact must be left to the geological speculator; but we may here mention some of the results of theory, which are fitted to make less formidable the idea of having a vast abyss of incandescent matter, within the comparatively thin crust of earth on which man and his works are supported. It results from Fourier's Analysis[*], that at 124 miles deep the earth may be actually incandescent, and yet that the effect of this fervid mass upon the temperature at the surface may be scarcely a perceptible fraction of a degree. The slowness with which any heating or cooling effect would take place through a solid crust, is much greater than might be supposed. If the earth, below 33 miles deep, were replaced by a globe of a temperature 500 times greater than that of boiling water, 200,000 years would be required to increase the temperature of the surface one degree[†]. A much smaller depth would make the effect on the superficial temperature insensible for 2000 years. It is calculated, moreover, that from the rate of increase of temperature in descending, the quantity of central heat which escapes in a century, through a square metre (about 3 feet 2¾ inches by 3 feet 2¾ inches,) of the earth's surface, would melt a column of ice, having that metre for its base, and 3 metres (9 feet 8 inches) high."—WHEWELL, *Report on Electricity, &c.; Dublin Session, British Association*, 1835.

Estimate of Solar Heat imparted to the Earth.

IT may be curious to contrast the effect of the heat ascending to the earth's surface, as stated in the article preceding, with that annually poured down upon it by the sun. This is estimated by Pouillet[‡] to be sufficient to melt a coat of ice, 14 metres thick, (about 46 Eng. feet,) incrusting the whole globe of the earth.

Progressive Rise of a Portion of the Bottom of the Mediterranean.

M. THEODORE VIRLET lately addressed a note to the French *Académie des Sciences*, in which he directs the attention of geologists to the probability of the speedy appearance of a new island in the Grecian Archipelago, in consequence of the progressive rise of a sunken solid rock (composed of trachytic obsidian?) in the gulf of the volcano of Santorin. The following are the author's observations on this subject:—"Towards the end of the last century, at the period when Olivier visited Santorin, the fishermen of the island asserted that the bottom of the sea had recently risen considerably between the island of Little Kaïméni and the port of Thera; in fact, the soundings did not give a greater depth than fifteen to twenty fathoms, where formerly the bottom could not be reached. When Colonel Bory and the author visited the island in 1829, they were able not only to confirm the truth of Olivier's statement, but also to ascertain, by various soundings, that the rise of the submarine land had continued, and that at the point indicated the depth was not more than four fathoms and a half. In 1830 the same observers made new soundings, which enabled them to determine the form and extent of the mass of rock, which in less than a year had been elevated half a fathom. It was found to extend nearly 900 yards from east to west, and about 550 from north to south. The submarine surface deepened gradually to the north and west, from four to twenty-nine fathoms; on the east and south this increased to forty-five fathoms. Beyond this limit the soundings indicated in all directions a very great depth. I have lately been informed that Admiral Lalande, who, since 1830, has twice returned to Santorin, has ascertained that the rock still continues to rise; and that, in September 1835, the date of his last visit, the depth of water amounted to only two fathoms; so that a sunken reef now exists which it is dangerous for brigs to approach. If the rock continues to rise at the same rate, it may be calculated that in 1840 it will form a new island. Since the eruptions of 1707 and 1712, which produced the new Kaïméni, volcanic phenomena have completely ceased in the gulf of Santorin, and the volcano seems at the present day quite extinct. Nevertheless, the rise of a portion of its surface seems to demonstrate continual efforts during fifty years to make an eruption; and that, whenever the resistance shall not be strong enough to offer a sufficient obstacle, the volcano will again resume its activity."

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[*] *Bull. des Sci.*, 1820, p. 58.

[†] *Mem.* Inst. Tom. vii. p. 603.

[‡] Tom. ii. p. 704.

French Scientific Congress, 1836.

THE Fourth Session of the Scientific Congress of France commenced on the 11th of September at Blois, and terminated on the 21st. About 200 members, natives of France, were present. There were several English, German, Belgic, and Spanish visiters, and, among the rest, several ladies.

The business of the Congress was pursued with such zeal, that, during nearly the whole of the session, several sections met twice a day, and very frequently the afternoon sitting was continued till late in the evening; but, notwithstanding, a day more than the prescribed period was required to bring up arrears, and was unanimously agreed to. The Fifth Session will be held at Metz in the early part of Sept. 1837.

Substitutes for costly Drugs.

"IT is well known that, in some instances, drugs which are vended under the same name, and might be supposed to be similar in their effects, are so different in their qualities and powers, that the administration of them is attended with much uncertainty, and, therefore, with considerable danger. The same prescription may, in cases exactly similar, produce, from the dissimilarity in the properties of the drug that is employed, either the cure or the fatal termination of the malady; and the accurate discrimination of the drugs which are genuine from those which are of inferior quality, is of the utmost importance to those by whom they are compounded, as well as to those by whom they are administered. I speak from very high authority when I state, that of some drugs which are vended in this country, only four parts in a hundred are of the best quality, and are consequently possessed of their full efficacy, while the other ninety-six parts contain, in some instances, only one-half the quantity of the active principles which ought to belong to them. I am assured, that of the colocynth imported into this country, only one-hundredth part is of the best quality; that there is of scammony, of the Peruvian bark, and of rhubarb, only a very small proportion; but that there is of ipecacuanha and of sarsaparilla a larger proportion, and a larger still of jalap; and all of these are, I need not say, very important medicines, which are frequently administered. If all of them were of an inferior quality, they would, of course, be far less efficacious, but there would not be the same difficulty and danger in employing them, as is now experienced from the inequality in their power, and, consequently, from the uncertainty in their operation.

"The great difference of price between those of the best and those of an inferior quality, offers a strong inducement to use the latter; and it would be an inestimable advantage to the art of medicine if satisfactory substitutes for them could be discovered amongst the plants which are indigenous to this country. An admirable paper of Dr. Rousseau* proves incontestably that in the cure of intermittent fevers holly is preferable to the Peruvian bark; there is reason to believe that rhubarb of a good quality could be produced in this country, and that elm bark may supply the place of sarsaparilla; and it deserves further inquiry, whether the juice of the EUPHORBIA *Cyparissias* could not be used instead of scammony, and the seeds of the ATRIPLEX *angustifolia* instead of ipecacuanha. The analysis of these common indigenous plants would be highly interesting."—EARL STANHOPE, *Address to the Medico-Botanical Society*, 1836.

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* Rewarded by this Society with their silver medal.

The Level of the Caspian Sea much below that of the Ocean.

IN 1814 Messrs Engelhardt and Parrot attempted to determine, by means of the barometer, if, as was long ago supposed, the waters of the Caspian Sea are less elevated than those of the Mediterranean and the Ocean. The mean of three determinations gave a difference in this respect of 320 feet. But subsequently M. Parrot having thrown some doubt on the result of the observations made in 1814, M. Erman has taken up the subject, and the following is the result of his investigations:—Barometrical observations made for seven years at Kasan, compared with corresponding observations made during the same period at Dantzig, give 104 feet as the height of the former town above the level of the Baltic. This result is confirmed by six years' observations at Mittau. Hence, with the assistance of levelling, M. Erman concludes that the height of the junction of the Kasanka with the Volga is only 29 feet above the Baltic. Thus, in order that there should be a coincidence between the levels of the Caspian and the Baltic, it would be necessary that, in the extent of 1470 miles, between Kasan and Astracan, the descent of the river should not be more than 29 feet, which seems inadmissible. The descent of the Volga from Torjok to Kasan, in an extent of 690 miles, has been measured. Supposing that in the remainder of its course the river follows the same law, M. Erman has ascertained that the depression of the Caspian Sea, compared with the Baltic, would be 275 feet,—a result which does not differ much from that (320 *feet) obtained by Messrs.* Engelhardt *and Parrot.*

Diamond-making anticipated.

ACCORDING to the following extract from a letter addressed by M. Theodore Virlet to M. Arago, another labourer in the vast field of chemical science appears to be approaching to the same point as Mr. Cross, but by a different route.

"Who does not know how many facts, perhaps among the most difficult to comprehend previously, have already been explained by the excellent researches of M. Becquerel in electrical chemistry, and the important labours of M. Fournet regarding the formation of veins? Numerous other facts, although not yet fully explained, have been brought forward, and admitted without dispute. For example, I have proved that the emery of Naxos comes from veins, and, consequently, had been formed, like the greater number of specular iron ores, by means of volatilization and sublimation; yet the corundum and oxide of iron, the mixture of which constitutes emery, are not more volatile than the carbonate of magnesia, which forms the subject of dispute.

"Since our chemical knowledge, then, does not always enable us to explain the phenomena whose existence we can prove, does it follow that we ought to call them in question? Has nature no mode of acting which surpasses our knowledge? And could she not proceed, for instance, by means of double chemical decomposition? On this supposition, the phenomenon (*dolomisation*) will admit of easy explanation. It is well known that all the muriates are volatile, or at least susceptible of sublimation. Magnesia might then easily reach the state of a muriate, and occasion the formation of a soluble hydrochlorate of lime, which would be carried off by the infiltration of water; while the magnesia, on the contrary, would be combined with that portion of the carbonic acid set at liberty, and would thus serve to form the double carbonate of magnesia and of lime, which constitutes dolomite, properly so called. In this there is certainly nothing inadmissible or contrary to reason, inasmuch as the hydrochloric acid gas is one of the gases most frequently disengaged from volcanoes, and the muriates ought to have been disengaged more abundantly in former times, if we admit, with geologists of the modern school, that the immense deposits of rock-salt which exist in saliferous formations are deposited by volatilization, in the midst of the strata which they penetrate.

"I am, therefore, of opinion that the modifications of rocks of the second class may henceforth be all explained by means of double decomposition,—a process which has enabled one of my friends, M. Aimé, to produce, in the laboratory, crystallized specular iron ore, analogous to that of the Island of Elba, as well as pure iron equally well crystallized,—a substance hitherto unknown to mineralogists: whence I conclude, *that the time is not perhaps far distant, when we shall be able to produce with ease all the species of precious stones, without even excepting the diamond.*"

Fossil Ferns.

THE following general conclusions regarding the geological and geographical distribution of fossil ferns, are contained in a recent Memoir by Professor Göppert. The beds of the coal formation contain the largest number of fossil ferns, viz. 183; while the muschelkalk, and the chalk and tertiary formations, contain the smallest number. The total number of these fossil vegetables at present known amounts to 253; of which 92 have been found in Silesia, 29 in Bohemia, 56 in the other countries of Germany, 49 in France and Belgium, 89 in Great Britain, 3 in Denmark and Sweden, 1 in Italy, 11 in North America, 1 in Holland, and 4 in the East Indies. The ferns that are the most widely distributed on the globe are the following:—*Alethopteris Serlii* (in England, France, Silesia, Pennsylvania), *Neuropteris angustifolia*, and *N. abutifolia* (in England, Bohemia, Silesia, Pennsylvania), and *N. Lohsii* (in England, France, Belgium, in the districts of the middle Rhine, in Bohemia, and Silesia). Most of the ferns of the Jura formation occur in England. The number of fossil ferns amounts nearly to a third of the total number (800) of fossil vegetables at present known. But it is very probable that we are acquainted with but a small portion of these fossils. Several genera of ferns belong exclusively to one or to two formations.

Fossil ferns, of all formations, without even excepting those of the chalk and the *Molasse*, present a striking resemblance to the tropical species of ferns, but none to those of temperate and cold climates. One of the principal conclusions to be drawn from the geological distribution of fossil ferns is, that each formation has particular species, which differ essentially from those of other formations. To this there are very few exceptions. Silesia is remarkable for its extremely rich fossil flora, for no less than 230 species have already been found in that country. The fossil flora of England resembles greatly that of Silesia. Excepting the genus *Stigmaria*, which is common to the transition and the coal formations, no species has been found in two formations. Finally, it is remarkable that dicotyledons and junci occur both in the most ancient and in the most modern deposits,—a fact which tends to prove that there is little foundation for the opinon that at the earliest epochs cellular plants only existed, afterwards monocotyledons, and then at a later period dicotyledons.

Light indefinitely produced.

M. CAUCHY had been led, theoretically, to anticipate, that near the limit of total reflection in a prism refraction takes place, with a *vast increase* in the intensity of the incident ray of light. He has since verified it experimentally. The most astonishing results may be expected to ensue from this extraordinary fact, since it would appear from it, that light, however small in quantity, may be magnified indefinitely.

Patent-Law Grievance. No. IX.

THE penalties inflicted on the inventive genius of Britain during the present year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £38,000!

N.B. This sum has been paid in *ready money*, on taking the first steps, and as many of the inventors are poor men, (operatives,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

NEW PATENTS. 1836.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

October *cont.*

241. Jeremiah Crook, Liverpool, *Lanc.*, Merchant; for improvements in the machinery for manufacturing hat bodies. Oct. 28.—April 28. *For. Comm.*

242. Thomas Edge, Great Peter-street, *Westm.*, Gas-apparatus and Lamp Manufacturer; for improvements in lighting or illuminating by gas, oil, or spirit-lights, or lamps. Oct. 28.—April 28. *For. Comm.*

Total, October...18.

November.

243. Robert Copland, Courlands, Wandsworth-rd., *Surry*, Esq.; for improvements upon patents already obtained by him, for combinations of apparatus for gaining power. Nov. 5.—May 5.

244. James Elnathan Smith, Liverpool, *Lanc.*, Merchant; for improvements in railways, and on locomotive carriages to work on such railways. Nov. 8.—June 8. *For. Comm.*

245. John Whitcher, Ringwood, *Hants*, Carrier; for improvements in drags or apparatus applicable to carriages. Nov. 8.—May 8.

246. James Smith the younger, and Francis Smith, Radford, *Nott.*, Mechanics; for improvements in certain machinery already known, for making bobbin-net, or twist lace. Nov. 8.—May 8.

247. Joel Livsey, Bury, *Lanc.*, Cotton Spinner; for improvements in machinery used for spinning, preparing, and doubling cotton and other fibrous substances. Nov. 10.—May 10.

248. Bertie Paterson, Peacock-street, Newington, *Surry*, Engineer; for improvements in the construction of meters or apparatus for measuring gas or liquids. Nov. 12.—May 12.

249. Henry Augustus Wells, New York, but now of Threadneedle-st., *London;* for his improvements in the manufacture of hats. Nov. 15.—Jan. 15.

250. Fletcher Woolley, York-st. East, Commercial-rd., *Middx.*, Gent.; for improvements in the manufacture or preparation of materials to be used as a substitute for bees'-wax, parts of which improvements are applicable to other purposes. Nov. 15.—May 15.

251. John Yule, Glasgow, Practical Engineer; for improvements in rotatory engines, or an improved rotatory engine. Nov. 15.—May 15.

252. Augustus Applegath, Crayford, *Kent*, Calico Printer; for improvements in printing calico and other fabrics. Nov. 15. —May 15.

253. Joseph Whitworth, Manchester, *Lanc.*, Engineer; for improvements in machinery for spinning and doubling cotton-wool, and other fibrous substances. Nov. 19.—May 19.

254. William Norris, Alston, *Cumb.*, Land Surveyor; for improvements in the manufacture of combs. Nov. 19.—May 19. *For. Comm.*

255. John Gordon Campbell, Glasgow, *Lanark*, Merchant, and John Gibson, of the same place, Throwster; for a new or improved process or manufacture of silk, and silk in combination with certain other fibrous substances. Nov. 19.—May 19.

256. John Buchanan, Ramsbottam, *Lanc.*, Millwright; for an improved apparatus for the purpose of dyeing, and performing similar operations. Nov. 22.—May 22.

257. Thomas Robson, Park-rd., Dalston, *Middx.*, Operative Chemist; for improvements in firing signal and other lights. Nov. 22.—May 22.

258. George Guynne, Holborn, Gent.; and James Young, Brewer, Brick-lane, *Middx.*; for improvements in the manufacture of sugars. Nov. 22.—Jan. 22.

259. Isaac Naylor, Stainsbrough, near Barnsley, *York*, Gamekeeper; for an alarum gun, or reporter and detector. Nov. 22.—May 22.

260. Timothy Hackworth, New Shildon, near Bishop Auckland, Engineer; for improvements in steam-engines. Nov. 22. —May 22.

261. Thomas Ellis, Stamford Hill, *Middx.*, Esq., and Thomas Burr, Shrewsbury, Shropshire; for improvements in the manufacture of sheets and pipes, or tubes, and other articles of lead and other metals. Nov. 24.—May 24.

262. Joseph Woollams, Wells, *Som.*, Gent.; for improved means of obtaining power and motion from known sources. Nov. 24.—May 24.

METEOROLOGICAL JOURNAL FOR OCTOBER, 1836; KEPT AT BLACKHEATH ROAD.

ay of Month	Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer		Daily Temp.	Solar Var.	Rad.	Clouds.		Clouds.		Direction of wind		Lunation.	WEATHER, &c.
					Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Satur. 1	29·500	58°	29·211	59°	37°4	55°1	46°3	17°7	35°	10	5	2·4	3	S.	S.S.W.		Rain heavy from 2 to 3 P.M., more than 1in. fell; clear
SUN. 2	29·362	56	29·475	57	41·2	51·0	46·1	9·8	39	2	8	3	1	W.S.W.	W. b S.	☾	Wind very high; driving showers; clear. [at sun-set.
Mon. 3	29·098	53	29·450	55	41·0	51·4	46·2	10·4	39	10	1	3	1	N.N.W.	S. W. S.		Violent squalls; afternoon more moderate; night clear.
Tues. 4	29·605	53	29·650	57	31·8	54·2	43·0	22·4	29	3	5	0	0	E.	E.		Very fine *cumuli;* evening much cloud and mistiness.
Wed. 5	29·959	53	30·019	57	33·6	55·5	44·5	21·9	32	2	2	0	0	N.	W.		Fog A.M.; *cumuli; aurora* early in even. [lightning.
Thurs. 6	30·001	56	29·864	57	38·7	58·8	48·8	20·1	35	10	10	0	1	E.	E.S.E.		Misty with light rain; rain, afternoon and even. with
Friday, 7	29·652	60	29·551	62	52·0	61·0	56·5	9·0	50	5	9	1	0	S. b E.	S.		Mostly overcast, with light rain in showers.
Satur. 8	29·524	62	29·516	63	52·6	59·0	55·8	6·4	50	8	5	1	2	S.	S.S.W.		Cloudy, drizzling showers, and close. [ning at night.
SUN. 9	29·506	61	29·525	62	43·8	56·5	51·0	12·7	41	3	7	2·3	2	S.W.	S.S.W.		Showers till sun-rise; *nimbi* with light showers; light-
Mon. 10	29·391	62	29·450	63	48·0	60·0	54·0	12·0	44	10	8	2·3	4	S.W.	S.S.W.	●	Rain till 10 A.M; windy; alternately clear and cloudy.
Tues. 11	29·348	62	29·516	62	51·4	59·6	55·5	8·2	50	5	1	5·4	2	S.W.	S.W.		Stormy night; wind violent till noon; fine night.
Wed. 12	29·747	61	29·674	62	43·8	57·2	50·5	13·4	40	5	9	2	4	S.W.	S.		Fine morn.; *cirro-stratus;* rainy even., stormy night.
Thurs. 13	29·251	61	29·460	63	48·6	59·0	53·8	10·4	45	9	6	4	2	S.W.	S.W.		Wind very high, showers and *nimbi;* evening cloudy.
Friday, 14	29·908	61	30·050	62	51·4	58·3	54·7	7·2	49	5	10	2	0	S.W.bW	S.W.S.		Fair; P.M. and night perfectly overcast; light rain.
Satur. 15	29·988	61	30·005	63	49·2	60·2	54·7	11·0	49	10	0	0	1	S.E.	S.W.		Rainy till noon; P.M. very fine, *cumuli;* clear night.
SUN. 16	30·303	60	30·332	63	40·0	58·5	49·2	18·5	38	10	10	0	0	N. b E.	E.		Misty morn, fine day, cloudy & misty night; *stratus.*
Mon. 17	30·333	62	30·302	63	50·0	58·2	54·1	8·2	50	10	20	0	0	E.	E.	☽	Thick *stratus;* sun not seen all day. [then cloudy.
Tues. 18	30·300	62	30·270	63	52·9	64·0	58·4	11·1	52½	10	7	1	0	S.	S.		Ditto; evening *cirro-cumuli;* fine red aurora till 9 P.M.,
Wed. 19	30·356	64	30·450	64	51·7	59·5	55·6	7·8	48	8	0	1	0	W.	N.W.N.		Rain before sun-rise; fine *cumuli;* clear night, with a
Thurs. 20	30·601	69	30·570	58	33·5	59·0	42·2	17·4	33	1	5	0	0	N.	N.E.		Misty; *cirro-cumuli* equally dispersed. [light mist.
Friday, 21	30·454	57	30·420	58	38·8	54·1	46·5	15·3	36	5	5	0	1	E.	E.		Misty; *cirro-cumuli; halo* round the moon at night.
Satur. 22	30·525	57	30·530	58	40·1	55·8	48·0	15·7	39	8	1	1	0	E.	E.		Cloudy; *cirro-cumuli;* very fine mild weather.
SUN. 23	30·539	56	30·501	59	35·3	54·0	44·6	18·7	33	10	0	1	0	W.	W.		Misty with *stratus;* afternoon and evening clear.
Mon. 24	30·533	58	30·482	58	40·2	53·5	46·9	13·3	38	10	10	1	0	W.	W.	○	*Stratus.* }
Tues. 25	30·396	58	30.349	57	48·0	49·9	49·0	1·9	47	10	10	0	1	W.	W.		Ditto overcast; *cirro-cumuli.* } Sun not seen.
Wed. 26	30·331	58	30·243	58	47·7	53·7	50·7	6·0	47	10	10	1	2	W.	W.		Cloudy throughout. }
Thurs. 27	29·802	58	29·959	58	47·5	52·0	49·7	4·5	47	10	1	3	2	W. b S.	N.W.		Overcast; violent squall at noon; clear frosty night.
Friday, 28	30·050	54	29·990	53	33·1	41·5	37·3	8·4	30	8	5	2	0	W. b N.	N.W. S.		Sleet A.M.; light *nimbi; cumuli;* even. clear. [in even.
Satur. 29	29·601	47	29·832	46	26·3	33·0	29·2	6·7	25	10	7	1	2	N.E.	N.N.E.		Deep snow at sun-rise; snow 10 till 2; freq. lightning
SUN. 30	30·150	44	30·170	46	26·2	37·1	31·7	10·9	23	1	1	2	1	N. b E.	N.N.W.		A little snow fell at even.; clear, sharp air; cloudy
Mon. 31	30·225	45	30·226	47	25·1	38·9	32·0	13·8	22	10	3	1	0	N.W.	N.N.W.		Overcast A.M.; fine day. [night.
Mean	29·947	57	29·969	58	41·96	53·92	47.92	11·96									

Bar. Max. 30·601 in. on the 20th. || Ther. Max. 64·0° on the 18th. || Lowest point of Rad. 22°, on the 31st. (*on the snow.*)
Bar. Min. 28·950 in. 3d. || Ther. Min. 25·1° 31st. || Rain and snow fallen 5·07 in.

SCIENTIFIC ALMANACS.

The Nautical Almanac; Connaissance des Tems; Astronomisches Jahrbuch (*Berlin*)*; Annuaire du Bureau des Longitudes; British Annual, &c.—for* 1837.

THE return of the season at which the host of almanacs makes its appearance, has this year been distinguished by that of the first number of a *British Annual*, published in imitation of the French *Annuaire*. This adoption of a plan which has been followed with such success for many years by our continental neighbours, seems to demand some notice in a scientific work like ours; and we shall take this opportunity of passing in review the principal almanacs, to enable our readers to form a judgment on their comparative merits, as well as on those of the stranger newly associated to them.

The original and essential purport of an *almanac* was to supply those in any way interested in astronomical pursuits with the relative position, during the current year, of the heavenly bodies, and with data for making calculations of their movements in the intervals between those periods for which the almanac furnished them. Immediately connected with this object, was the notice of future phenomena of occasional recurrence, such as eclipses, transits, occultations, &c. The importance to society generally, of everything connected with the *calendar*, or with the measurement and division of time, as immediately dependent on the apparent motions of the sun, &c. seemed to necessitate the addition of information on this subject, to the purely astronomical details originally contemplated. The intrusion of extraneous matter once allowed, the regular republication of a work with such contents, offered an opportunity for adding any other which might be generally interesting, that was of a periodical character; almanacs, accordingly, became a *mélange* of scientific, civil, and political notices, the two latter having no other connexion with the first than that of being annually modified.

We will not touch on that melancholy and humiliating chapter in the moral history of mankind—and the more painful because it is not yet finally closed*—which narrates the progress of credulity and imposture,

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* We observed, but yesterday, a placard announcing the recent publication of a work entitled *Phrenology and Astrology harmonized, showing that the compartments of the head, as divided for phrenological study, exactly agree with the astrological houses of Heaven, &c. &c.*

The following passage from a cotemporary journal, published before the repeal of the duty on almanacs, presents some remarkable statements on this subject:—

"The total number of almanacs published may be divided into the astrological and the non-astrological. The astrological are published *by the Stationers' Company only. There was a third class of the company's* almanacs—the obscene; but this class was discontinued in 1829. (!)

"There are now only two astrological almanacs, *Vox Stellarum, by Francis Moore, physician*, and *Merlinus Liberatus, by John Partridge*. Moore's *improved* almanac has this year (1832) ceased to belong to this class. These two contribute, there is good reason to believe, one-half of the revenue upon almanacs—that is, they sell 250,000 copies. Of these again, nine-tenths of the number may be put to the account of Francis Moore. So that this relic of ancient absurdity is probably more read than any other work in the kingdom.

"John Partridge commenced his vocaattaching themselves to a science that would otherwise have earlier exalted our nature, by the contemplation of eternity, of space and duration, viewed through a medium uncontaminated by their breath; and which describes them as working their baleful spells under the arrogated sanction of her name, and gradually increasing their influence till it blinded the majority of the human race to truth and reason. Most almanacs, as our readers know, were formerly the vehicles for the dissemination of much of the poison we allude to; and, perhaps, the jealous anxiety to rescue the true original object of their institution from any portion of this disgraceful stigma, may have been a powerful, though unacknowledged, reason for again separating the purely astronomical part of their contents from the miscellaneous matter which was being continually engrafted on the parent stock: a separation, however, sufficiently accounted for by the demands of an extending knowledge in the science, and in the dependent art of navigation, which required a greater space being devoted to their use than was compatible with the admission of anything not essentially connected with the subject. This separation accordingly has taken place in most European nations, and in the United States, but it is to the three leading astronomical almanacs of the old world, that our remarks will be chiefly confined.

Our own national almanac and the French *Connaissance des Tems* undoubtedly stand at the head of all these works. We place them

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tion as an almanac-maker soon after the restoration; Francis Moore began his career of imposture in 1698; Partridge, therefore, has the advantage of senility over his rival, and that ought to go a good way in balancing the relative merits of their stupidity. It is probable, however, that Partridge's almanac never entirely made head against the wicked wit of Swift; for it is a remarkable fact that Bickerstaff killed this identical almanac for a season, and frightened the real Partridge from attempting to set it up again. The Stationers' Company, however, were not to be so beaten out of a profitable imposture, and they had the impudence, in 1714, to publish a *Partridge's Almanac*, with a portrait of the seer, which the worthy man refused to acknowledge. The defeated astrologer obstinately persisting not to prophesy in the flesh, the company continued to employ the *ghost* only of Partridge, and the work even now bears the motto, *Etiam mortuus loquitur.* This original schism, and the acknowledgment of the death of the almanac-maker, is the only reason we can assign for Partridge not being as popular as Moore. He is unquestionably as silly." (*Quarterly Journal of Education. No. V.*)

The name of Partridge was assumed; the compiler's real one was Gadbury, a most prolific writer, to judge by the number of his works extant in the British Museum, all treating of astrology, &c.

It must not be supposed that England stands alone in this disgraceful position. Moore's almanac was, till lately, if not is, annually reprinted in Paris and in Boulogne, and similar evidences of barbarity annually make their appearance at Liège, Coblentz, &c. &c. At home, the sale of this work is chiefly confined to the agricultural population, the least enlightened in all stages of society. Mr. Baily, in alluding to the endeavours made by the respectable editors of these kinds of almanacs to purify them gradually from the nonsense they contain, states, that such an endeavour was made with regard to Moore's almanac, by omitting, one year, the column of influences of the moon on parts of the body, and that nearly the whole impression of 100,000 was, in consequence, returned by the buyers on the hands of the publisher, as defective.

The *worshipful* company mentioned in the above extract advertise this year (1837), in their annual list of eighteen almanacs, *Francis Moore, price 6d.—Moore's Almanac Improved*, *9d.*; the improvement for which the extra price is charged consisting chiefly of the *omission* of the predictions, &c. In the same list, Partridge's almanac still holds the third place, the post of honour being assigned to the genuine Moore, in consequence, we fear, of its being still the most profitable.

together, because they have advanced to their present degree of perfection in consequence chiefly of the reciprocal effect they have had on each other. If the *Connaissance des Tems* can lay claim to seniority of birth, the volume for 1837 being the hundred and fifty-ninth of a series which has never suffered interruption since its commencement in 1679 by Picard, yet we suspect it would no more have attained its maturity without the spirit of emulation excited by the subsequent appearance of our *Nautical Almanac*, than this would, if it had not been for the beneficial example of such a pre-existing model, and for the assistance derived from the authentic sources of means for calculation, so liberally communicated by the French astronomers to ours.

The *Nautical Almanac* was the fruit of the exertions of the late Dr. Maskelyne in the cause of his favourite pursuit. That distinguished practical astronomer, in the course of several voyages undertaken by him for scientific purposes, had ample opportunity for remarking the difficulties the navigator laboured under, owing to the want of proper works on nautical astronomy: he presented a memorial to the Commissioners of Longitude on February 9th, 1765, in which he proved, by facts, the utility of the method of deducing the longitude by means of the moon's distance from the fixed stars, as it had been promulgated by him in a work he had recently published for that purpose, called *The British Mariner's Guide:* he then stated the necessity that existed for a good nautical ephemeris to enable the full benefit of this method being reaped. The board having heard the evidence of four naval officers as to the advantages of the new method of "lunar distances," came to a resolution that the "tables of the late Professor Mayer should be purchased of his widow, and printed; and that a nautical ephemeris should be compiled to use with them." The superintendence of the latter labour was confided to Dr. Maskelyne, who had then become astronomer royal, and he exerted himself so actively on the occasion, that the first number of the *Nautical Almanac* for the year 1767 was published in 1766, accompanied by a preface written by him, describing the use and construction of the tables, and stating those from which the calculations had been made*.

The work appeared regularly with little or no alteration in its general contents and arrangement, for upwards of sixty years†, except that the calculations were made from every improved formula, or set of tables, that was produced in succession during that period, either abroad or at home; the introduction in the number for 1822 of the apparent places of twenty-four principal fixed stars, which were increased to sixty in that for 1827, and the extension in that for 1833 of the table of the moon's right ascension and declination to every *third* hour of the day, which had been previously given for noon and midnight only; these important improvements being effected by the late Mr. Pond, in the official exercise of his duty of superintending the publication, as Dr. Maskelyne's successor.

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* Dr. Maskelyne did not confine his labours to the publication of the Almanac; a series of *tables, requisite* to be used with it, appeared at the same time, drawn up by him; this valuable work, which has ever since been known by the above quaint title, has gone through numerous editions, each being improved on that which preceded it.

† In the first *Nautical Almanac* for 1767, the column of differences of the sun's declination, which appeared subsequently from about 1800 to 1833, was not given. In the earliest numbers the tables *of the five* planets then known *were only given for every* six days of each month; these were increased, for Mercury, to every third day, before 1800; but all the rest remained as before, except that the right ascension of the planets was added soon after the year 1810. In the almanac for 1833, several additions were made; among others, the times of the rising and setting of the sun's and moon's centres. These columns have disappeared again since that year, in order to make way for more *generally* valuable matter, contrary, however, to the practice of the *Connaissance des Tems* and the *Astronomisches Jahrbuch*, both of which works give these times.

Towards the year 1830, the attention of Government was drawn, by the urgent representations on the subject which appeared in print, to the comparative inefficiency of the national almanac, which had not kept pace with the rapid extension of astronomical knowledge, the improvement in instruments, and the increased skill of our navigators, and which had been for some years surpassed by continental works of the same kind, published under the superintendence of the first astronomers of the day. The Lords of the Admiralty, accordingly, at the instigation of the Board of Longitude, in that year requested the aid of the Astronomical Society; a general committee of forty members was in consequence appointed, ten of the most pre-eminent of which formed a managing sub-committee. This committee drew up and presented to the Admiralty, in the November of the same year, an elaborate report, suggesting those extensive alterations and additions, by the adoption of which the work has been rendered the most complete astronomical ephemeris published. About the same period, the superintendence of its compilation and publication, after several changes, was finally confided to Lieutenant Stratford, with the assistance of Mr. Woolhouse and an efficient *staff* of calculators, revisers, &c.

It is obvious that the utility to mariners of such an almanac must in a great measure depend, not only on the accuracy with which the calculations are originally made, but on its exemption from typographical errors when printed. The former object is secured "by duplicate calculations of the most important tables, such as all those relating to the moon, the occultations of the eclipses of Jupiter's satellites, &c. and in every instance by some independent calculations, to guard against errors in principle; and the results are finally examined by means of differences." The latter object can only be attained by a revision of the sheets in going through the press, made with a care far exceeding that required for any other work[*]. The pains taken in this respect by the gentlemen who have the management, is shown by the fact, that in the volume before us, consisting of 496 pages of figures, there are only two errata as yet discovered, of not the slightest consequence in themselves, but important as proving the care bestowed on the revisal. In the volume for 1836, there were seven errors; in that for 1835 there were ten, the majority of which were errors in noting down the results of the computations[†].

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[*] The process of stereotyping, when applied to printing standard works of numerical computations, such as tables of logarithms, &c. ultimately secures perfect accuracy of typography, since any error can be corrected in each successive reprint without the possibility of the occurrence of a new one; but an almanac is seldom, if ever, stereotyped, since, except for reference, its utility is confined to the current year.

[†] The Astronomical Society, in the report, art. 26, "strongly recommend that

Among the methods adopted for securing distinctness as well as correctness was the having a new fount of figures cast for the purpose, the matrices being cut with every attention to precision, in preference to mere beauty, of form; the 6 is made so that if inverted it would not look like a 9, but would immediately catch the eye of compositor and corrector, the identity of these two numerals when either is inverted having ever been a fertile source of error in printing arithmetical tables. With the same view, all unnecessary use of dots and commas has been avoided; degrees, minutes and seconds, are not separated by commas, as we do pounds, shillings and pence in our notation, nor by a full stop, as is still the case in the *Connaissance des Tems*, but by a sufficient space being left between them to prevent the possibility of confusion, and the decimals are separated from integers by a full point at the top of the line, thus, 2·73, and not by a comma at the bottom, thus, 2,73, as is universally the case in French and German arithmetical notation. Minutes and seconds of *time* are marked thus, $17^{h}\ 26^{m}\ 37^{s}{\cdot}53$, to distinguish them from $17^{\circ}\ 26', 37''{\cdot}53$, minutes and seconds of degreees, an improvement not adopted on the continent; but which, trifling and useless as it might appear, if the work were intended for literary and scientific readers only, is valuable in one intended for masters and mates of vessels, who ought to be spared every unnecessary exertion of thought, in order to guard against errors in calculations, for which they have little time, and fewer conveniences. On the whole, few works could be referred to which would place in so striking a point of view the excellence to which the arts relating to typography are carried in England; the beauty of the paper, the exquisite symmetry and clearness of the *forms*, and the taste in their arrangement, present a favourable point of comparison with the French almanac, which nevertheless is *got up* with great care*.

It has always been an important object to publish the *Nautical Almanac* some years in advance, that captains of vessels about to proceed on distant voyages might take with them the numbers for the years they would, probably, be absent. From the commencement till 1832, this was regularly accomplished, the work being never less than three complete years in advance, but the delay required to carry into effect the suggestions of the Astronomical Society, and that caused by the changes in the management of the publication, prevented the improved

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any errors discovered in the *Nautical Almanac* should be printed immediately for general information, and be annexed to all the unsold copies; and that notice of the same be advertised in the *London Gazette*, and in some of the public papers, as early as possible." We believe that this, like every other recommendation of the society, has been strictly attended to, and that any communication on the subject will be thankfully received at the office in Somerset House.

* Being strictly a national work, the *Nautical Almanac*, to place it within the means of the greatest number of persons who may need it, is sold at a price that could not now remunerate a private publisher, nor even as we should think cover its expenses; the volume, with an Appendix containing 569 large octavo closely-printed pages of the most expensive form of work, is sold for *five shillings*. The *Connaissance des Tems*, containing 500 equal and similar pages is priced *seven francs*, about *six shillings*. The *Astronomisches Jahrbuch*, containing 340 pages, costs *thirteen shillings*.

The *Nautical Almanac* in 1767 was sold for half a crown. In 1802 it was raised to five shillings, its present price, though it now contains more than three times the quantity of matter it formerly did.

volume for 1834 from appearing more than a few months before the commencement of that year; the exertions of the present managers have, however, partly redeemed the lost time, and the volume for 1838 is now (December, 1836) published; but this is not sufficient; the Astronomical Society recommend in their report, that it should be always *four* years in advance, and it is to be hoped, that no remission of exertions will be allowed till this is accomplished.

We shall now give our readers a brief account of what the *Nautical Almanac* contains, marking in italics, or mentioning in a note, those additions and alterations which were effected for the first time in the number for 1834, in pursuance of the recommendation of the Astronomical Society. Two preliminary pages contain the usual articles of the calendar, festivals and anniversaries, &c. with the number of the *year according to the Jewish and Mohammedan eras, and the date of the commencement of the Ramâdan*[*].

The ephemeris for each month consists of twenty two pages, divided into columns, their contents being as follows:—

P. I., col. 1. Day of the week; 2. Day of the month; 3. The Sun's right ascension, given in time, to *hundredths of a second; 4. The difference for one hour*, to facilitate the reduction of the quantities from the meridian of Greenwich to that of any other place; 5. The sun's declination, to *tenths of seconds; 6. Difference for one hour; 7.* Sidereal time of the semidiameter's passing the meridian, *to hundredths of a second;* 8. Equation of time to do.; 9. Difference for an hour. All for *apparent* noon.

P. II., cols. 1, 2. *Day of week and month*; 3, 4. Right ascension and declination; 5. Semidiameter, to *tenths of seconds; 6. Equation of time; 7.* Sidereal time, *to hundredths of seconds.* All for *mean* noon.

P. III., col. 1. *Day of month;* 2. Sun's longitude at noon, in degrees, to *tenths of a second*[†]*;* 3. Sun's latitude at noon, to seconds and hundredths; 4. Logarithm of the earth's radius vector at noon, to seven places of figures; 5, 6. The Moon's semidiameter for noon and midnight; 7, 8. Her horizontal parallax for do. do.; all to *tenths of a second.* This and all the following pages, to the eighteenth inclusive, are for *mean time.*

P. IV., cols. 1, 2. Day of week, month; 3, 4. Moon's longitude for noon and midnight; 5, 6. Her latitude for do.; 7. Her age at noon, in days and *tenths;* 8. Her passage of the meridian, in hours, minutes, and tenths.

PP. V. to XII., inclusive, contain the Moon's right ascension and declination, for *every hour* of every day of the month; the former, to seconds and hundredths of time, the latter, to seconds and *tenths* of degrees; and a *third column for the difference of declination for ten minutes, in seconds and hundredths.* A corner of the XIIth page, unoccupied by the foregoing is filled up with the times of the moon's phases, and the times of her apogee and perigee.

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[*] These two pages might be more economically and usefully occupied than they are at present. Why not give the Mohammedan and Jewish calendars, as the two continental works do, and the corresponding dates in the different eras, as we find them in the *British Almanac*? This kind of information would be more in accordance with the purely scientific character of the *Nautical Almanac* than the University and law terms, the birthdays, coronations, restorations, gunpowder-plots, &c.

[†] The use of the division into signs has been discontinued; formerly the longitude would have been given as $9^s\ 10^\circ$, &c. now it is given as 280°, &c.

PP. XIII. to XVIII., inclusive, contain the "LUNAR DISTANCES," or distances of the moon's centre, for every three hours of every day of the month, from that of the sun, and from three to six principal stars, according to circumstances, in degrees, minutes and seconds; a column is appended tothat for each triad of hours, to contain the proportional logarithm, to four figures, of the differences of the distances for that interval[*].

P. XIX. contains the CONFIGURATIONS OF JUPITER'S SATELLITES, for each day of the month, or diagrams of the satellites, as they would appear, disregarding their latitudes, in an inverting telescope; and each diagram shows by a lateral addition which satellite is on the disk, or which is eclipsed by the planet or its shadow, whenever an occultation occurs.

P. XX. contains the mean and sidereal time, to tenths of a second, at which all the ECLIPSES OF JUPITER'S SATELLITES occur, during the month, those which are visible at Greenwich being distinguished by an asterisk. *There is a diagram annexed to the division for each satellite, which represents the proportional distance from the disk of the planet at which the immersion into, or emersion out of, the shadow occurs.*

P. XXI. contains the approximate sidereal times of all the OCCULTATIONS OF JUPITER'S SATELLITES by the planet, and of the transits of the satellites and their shadows; *the times of the immersion and emersion of the former, and of the ingress and egress of the latter, are given in separate columns to minutes*, those which are visible at Greenwich being distinguished by an asterisk as above.

P. XXII., col. 1. Day of month; 2, 3, 4, 5. the logs. to four figures of four factors, designated A, B, C, and D, used in correcting the places of the fixed stars, according to a formula of Professor Bessel, and calculated for mean midnight. 6. *Mean time of the transit of the first point of Aries, to hundredths of a second*, showing the distance of the *mean* sun from the meridian, at the instant when the *true* point of intersection of the ecliptic and equator (called the first point of Aries) is on the meridian of Greenwich, for every day of the month. 7. *Mean equinoctial time, in mean solar days and the fractional part; or the mean time elapsed since the instant of the mean vernal equinox, for mean noon for every day of the month*[†]. 8, 9.

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[*] There have appeared, on an average, 13,000 of those distances yearly in the newly modified almanac.

[†] This species of time was first introduced in the Supplement to the *Nautical Almanac* for 1828, with a very full explanation of its nature and use written by Mr. now Sir J. Herschel. It there appears that the use of equinoctial time is to afford an uniform date, which shall be independent of the different meridians, and of all *inequalities in the sun's motion*; and *shall thus save the necessity*, when speaking of the time of any event, of mentioning the *place* where it was observed or computed. Thus it is the same thing to say, that a comet passed its perihelion on January 5th 1837, at $5^{\mathrm{h}}_{\mathrm{h}}$ 47^{m} $0^{\mathrm{s}}\cdot0$ mean time at Greenwich; at 5 56^{m} $21^{\mathrm{s}}\cdot5$; mean time at Paris; or at 1836^{y} 289^{d} 6^{h} 16^{m} $40^{\mathrm{s}}\cdot96$ equinoctial time; but the former dates make the localities of Greenwich and Paris enter as elements of the expression, whereas the latter expresses the period elapsed since an epoch common to all the world, and identifiable independently of all localities. By this means, all ambiguities in the

contain the day of the year, and *fraction of the year, to three decimal places, from mean noon of January* 1.

Immediately following the ample ephemerides for the twelve months above described, we find a page containing the APPARENT OBLIQUITY OF THE ECLIPTIC to *hundredths* of a second, for every tenth day throughout the year, with the mean obliquity for January 1, $=23^{\circ}\ 27'\ 37''\cdot89$; the remaining columns of the page contain, the sun's horizontal parallax, and *aberration, in seconds and hundredths;* the equation of the equinoxes in longitude (in seconds of degrees), and of right ascension (in seconds of time), to *hundredths;* and lastly, the mean longitude of the moon's ascending node, in degrees, minutes and tenths; all for every tenth day of the year.

Then follow the ephemerides of MERCURY, VENUS, MARS, JUPITER, SATURN and URANUS (here still most barbarously called "the Georgian[*]"), a page being devoted to each, for each month of the year; this page containing the geocentric right ascension and declination, in time and degrees, to *hundredths of seconds;* the log. of the earth's distance, to *seven* places; and the time of the passage of the meridian, to *tenths* of seconds; the heliocentric longitude and latitude, to *tenths* of seconds of degrees; and the log. of the radius vector. All for the mean noon of *every day* of the month[†].

The same complete data for VESTA, JUNO, PALLAS, and CERES, *are given, but only for every fourth day of the year; during the time of their opposition, however, the right ascension and declination, and logarithms of the distances from the earth and sun, are extended, in separate ephemerides, to every day, for three months, for mean midnight.*

The equatorial, horizontal parallax, and semidiameter, of the six principal planets are then given, to hundredths of seconds, at mean noon, for every fifth day of the year.

The mean places of 100 principal FIXED STARS for the first day of the year, next meet our view, in six columns; the first, containing the name of the star, with its popular Arabic or classical name (annexed between brackets, and in italics), while the standard stars are distinguished by being printed in capitals; the 2. col. gives their magnitudes;

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reckoning of time are supposed to be avoided.

To convert mean solar time into equinoctial time: to the corresponding Greenwich mean time add the equinoctial time at mean noon of the same day at Greenwich; the sum will be the equinoctial time required. Thus, in the instance of the comet before alluded to; Paris being 9^{m} $21^{s}\cdot5$ east of Greenwich, subtract this from the Paris time, and we get 5^{h} 47^{m} $0^{s}\cdot0$ for the corresponding Greenwich time, to which add $289_{d}\cdot020613$ or 288_{d} 0_{h} 29^{m} $40^{s}\cdot96$, the mean equinoctial time at Greenwich, mean noon on January 5th, and the sum will represent the mean equinoctial time of the comet's passage of its perihelion, viz., 289^{d} 6^{h} 16^{m} $40^{s}\cdot96$ from the vernal equinox of 1836. (*Nautical Almanac*, 1837. *Explanation*, p. 513.)

[*] *The* privilege of conferring a name on a new discovery by the finder, was certainly never more properly violated than in this instance; in compliment to the astronomer, the planet was for some time called Herschel, but the substitution of the classical *Uranus*, to assimilate with the titles of the rest of the heavenly host, for the unfortunate designation of *Georgium*, is really indispensable.

[†] It is worth remarking that throughout the work, in each month, the day is given which really constitutes the first of the next; thus, in the months with 31 days we find the data for the 32d day of the month, and in those with 30 days, we have them for the 31st; these data are, therefore, of course, repeated twice, at the end of the one and the beginning of the next month. This is done to facilitate the taking of differences, by obviating the necessity for turning over the leaf when one quantity is for the last day of one, and the other for the first day of the next month.

the 3. their right ascension, in time, to three places of decimals of seconds; 5. their declination in degrees, to two places of seconds, while the 4. and 6. cols. contain the annual variation to four decimal places of seconds, for each of these elements respectively.

In p. 365 we find the formulæ of reduction according to Professor Bessel, followed by the "constants for facilitating the reduction of stars" required in the foregoing formulæ, calculated for mean midnight at Greenwich, for every fifth day of the year.

The right ascension and declination of α Ursæ minoris (Polaris) and of δ Ursæ minoris are next given for every day, while those of the remaining ninety-eight stars are given for every tenth day, of the year, to hundredths of seconds in time and degrees respectively. To these are appended tables of corrections to be applied to the apparent places of five polar stars, for the terms of nutation, the argument being the moon's longitude.

The table of forty-one pages that follows, contains what are termed MOON-CULMINATING STARS*; showing, the magnitude, apparent right ascension, declination, variation of the moon's right ascension in one hour of longitude, and the sidereal time of the passage of the meridian of her semidiameter, all given to hundredths of seconds in time or degrees, respectively; in separate columns for every day of the year, and calculated for the Greenwich meridian.

The next table is that of the OCCULTATIONS OF THE PLANETS AND FIXED STARS, by the moon, which are visible at Greenwich during the year; the sidereal and mean time of the immersion and emersion being given in hours and minutes, with the angle from the North point, and from the vertex of the moon's disk, in degrees, of the point at which the immersion or emersion takes place, in separate columns respectively.

This is accompanied by an elaborate table of the elements for facilitating the computation of the foregoing occultations, consisting of the apparent mean time of the conjunction in right ascension of the moon and stars; and of the apparent right ascension of moon and star, apparent declination of star, and difference of apparent declinations of moon and star, at the mean time of conjunction, computed for Greenwich; and lastly, the *limiting parallels*, or the parallels of latitude beyond which an occultation cannot possibly be visible.

The next section of the almanac contains the PHENOMENA of the year, commencing with the solar and lunar eclipses, giving all their elements, and every particular regarding them. Next, the mean times of the conjunctions, quadratures, oppositions of the sun, moon, and planets, with each other, or with certain fixed stars; and of the *passage of the nodes*, of the aphelion and perihelion, greatest elongations, &c. that occur in each month, the latitude of one of the bodies being given; as well as the times when the planets are in the positions most favourable for observation; the comparative intensity of light of the four asteroid planets being also recorded at these times.

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* They are so denominated as "being near the moon's parallel of declination, and not differing much from her in right ascension; they are proper to be observed with the moon, in order to determine differences of meridians. This is effected by comparing the differences of the observed right ascensions of such a star and the moon's bright limb, at any two meridians. If the moon had no motion, the difference of her right ascension from that of the star would *be constant at all meridians*; but in the interval of her transit over two different meridians, her right ascension will have varied, and the difference between the two compared differences will exhibit the amount of this variation; which, added to the difference of the meridians, shows the angle through which the westerly meridian must revolve before it comes up with the moon; hence, and knowing the rate of her increase in right ascension, the difference of the longitude may be easily obtained." (Explanation, p. 519.)

Then follow, *The elements for determining the geocentric position, magnitude, and appearance, of Saturn's ring.* A table showing the mean time of the greatest libration of the moon's apparent disk, as it occurs twice, at least, in each calendar month. *A table showing the illuminated portions of the disks of Venus and Mars, in parts of the diameter of the disk considered as unity*, to three places of figures, for the middle day of each month.

We then have the ephemeris of the stars proper to be observed with Mars near the opposition of that planet on February 5th next.

A table of high water, at London Bridge, for every day of the year, and another of that, at the full and change of the moon, at 200 principal ports on the coasts of the British islands, or the opposite ones of France, Holland, &c.

A table showing the correction, in seconds, required, on account of second differences, in finding the Greenwich time corresponding to a reduced lunar distance.

Three tables for determining the latitude by observations of the pole-star, when not on the meridian.

A table for converting intervals of mean solar, into equivalent ones of mean sidereal, time.

And lastly, the latitudes and longitudes, to tenths of a second, of the principal observatories from the meridian of Greenwich, with the authorities from which they are taken.

These luminous, ample, and invaluable data are followed by an "Explanation," in 29 pages, of their use and application, illustrated in every case by examples, written simply, perspicuously, and most carefully, to render them intelligible to those who may require such assistance. We look upon these " explanations" as forming a brief system of astronomical calculations, and indeed, in a great measure, of practical astronomy generally, that it would be difficult to equal, and, together with the almanac preceding them, as proving, that the management of the work could not have been placed in more competent hands.

The Appendix to the *Nautical Almanac*, which with those of the former numbers since 1833, are intended to be separated and formed into a distinct volume, contains an elaborate paper by the Astronomer Royal, " On the calculation of the perturbations of the small planets, and comets of short periods;" and a paper of Mr. Woolhouse's, " On the determination of the longitude from an observed solar occultation or eclipse." But we cannot enter into any consideration of such purely mathematical papers in our magazine.

From this account of the contents of our national ephemeris, our readers may infer, that neither care nor cost has been spared in order to reduce, as much as possible, the trouble of computation to practical astronomers, and especially to mariners. The French government have not gone to nearly the same length in their *Connaissance des Tems;* but, perhaps, the system of scientific instruction given to all persons intended for the public service, and the facilities afforded by numerous schools in every department, and by the best elementary works on all sciences, to the acquisition of the same knowledge by private individuals, may be considered as a satisfactory answer to those who might be disposed to think slightly of the French national almanac. However this may be, the difference between the two works, as regards the extent and number of the tables, is most striking.

In the sun's ephemeris for each month, the rising and setting for mean time; the mean right ascension; the longitude; the true right ascension and the declination, for mean noon, with the differences *for the whole day*, and the equation of time, and a column of differences, are all that are given; but there is no latitude, no double column of equation of time, nor of sidereal time; while the sidereal and mean time of the semidiameter passing the meridian, the log. of the radius vector, and the hourly motion in longitude, as well as the semidiameter, are only given for every five days of the month.

In the ephemeris for the moon we miss the ample table of her right ascension and declination to every hour of the day, but we have the times of her rising and setting.

The "lunar distances" are not nearly so well arranged as in the *Nautical Almanac*, and the same system is pursued of only giving the total differences, which can be taken at a glance by the reader, instead of furnishing him with proportional logs. or proportional differences.

The monthly ephemerides of the planets are extremely meagre compared to ours, being only for every 3rd, 6th, 8th, 10th, and 15th day of the month respectively, for Mercury, Venus, Mars, Jupiter, Saturn, and Uranus; their rising and setting is furnished as usual.

The diagrams of the configurations of the satellites of Jupiter resemble ours, but the tables of the eclipses are far from being so copious.

Immediately following the regular monthly ephemerides, we meet with a table of *Phénomenès et Observations* which at first sight seem greatly to surpass ours in contents, not more than eighteen on an average being given in the *Nautical Almanac* for each month, while the *Connaissance des Tems*, by combining under this head the moon's occultations of the fixed stars and planets, makes a greater show, and, as far as we can see, really contains more of these last than are given in the English work in the separate table devoted to the occultations.

The apparent positions of seventy-six fixed stars are given, but not so completely as by the *Nautical Almanac*, but subsequently another of one hundred, for 1830, after Piazzi's catalogue, partly supplies the deficiency.

The French almanac possesses, on the contrary, a very full set of tables of the distances of the moon from Venus, Mars, Jupiter, and Saturn, arranged by themselves; which appears to us perhaps a better plan than mingling these distances from the planets with those from the fixed stars; but the same columns of total differences between the given *diurnal distances* again strike us as occupying much space that might be better filled.

Two tables of refraction, for correcting observed altitudes; two of logs. for correcting those of the sun and stars; a table of second differences; others for converting time into equatorial longitude, and the reverse; and for reducing sidereal into mean time, and the reverse; and a table of solar parallax for different altitudes, and at different seasons.

And lastly, a most copious table of latitudes and longitudes, the latter both in degrees and time, for most of the places on the earth's surface, classed in sections, according to the continent or country in which they lie, and with the authority for each, closes this part of the work.

The "Explanation" is followed by a valuable and lucid meteorological table for the year 1833, from observations made at the Paris observatory. The *mean* heights of the barometer, reduced to the temperature of melting ice, for nine A. M., noon, three P. M., and nine P. M., being given for every month, as are those of the centigrade thermometer for the same hours. The table specifies the number of days of rain, of wind, of fog and mist, of frost, of snow, of hail and sleet, of thunder, of aurora borealis, and the mean quantity of rain for every month of the year, with the temperature of the centigrade thermometer in a vault, for one day of each month*.

The *Astronomisches Jahrbuch* was commenced sixty-two years ago, by the late Professor Bode, and was conducted by him to the time of his death with the most unremitting attention. When Professor Encke succeeded Bode, in 1830, he remodelled the ephemeris, augmenting its contents to suit the advanced state of astronomical knowledge; to admit of this, without greatly exceeding the prescribed limits, he discontinued the elaborate appendix, which had no immediate reference to the work as an annual almanac, but consisted of miscellaneous papers on astronomical subjects; these papers now find their fitting receptacle in Professor Schumacher's *Astronomische Nachrichten*. Nevertheless, the present number for 1837, like our own and the French almanacs, contains two learned mathematical papers, one on *interpolations*, and the other on the calculation of *periodical disturbances;* and a table of the co-ordinates of the sun, X, Y, and Z, for every second day of the year.

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* The table is not accompanied by any explanation, which is the more to be regretted, as some of its statements seem particularly to demand one; thus in the column of the *days of wind* (jours de vent), we find *every day of the year, without exception*, and the total, 365, given separately, proves that it is so meant; as we cannot suppose that Paris never enjoyed a calm day for the whole year, we presume that any, the slightest, current of air, is considered as a wind. If we were to take the year 1833, as chronicled in this table, for an average, our friends the Parisians have no reason to triumph over us, for it appears that there was in that city *a fog, or a mist, on every day of the first three months*, and *on seventeen* of the fourth. The total *number of foggy days for the year being 168;* of rain, 164; of frost, 45; of snow, 11; of hail, 10; of thunder, 9; and of aurora, 1, viz. on the 12th October.

The columns of the quantity of rain which fell in the court, and on the top of the Observatory, present the difference in this quantity at different elevations, in a striking point of view, the quantity in the former situation of the gauge, for the whole year, being 580·35 millimetres, (22·83 inches,) while that in the latter was only 487·1, (19·16 inches,) being a diference of 3·66 inches.

The equality of temperature in the vault is also very remarkable, it never varying more than ·005° C. (·008 F.), and for eleven months not more than half that minute quantity; while in the open air, the mean temperature for the year, at 3 P. M., varied from 31° Fahr. in January, to 74° Fahr. in June.

The contents of the monthly ephemerides hold an immediate place in point of comprehensiveness between the *Connaissance des Tems* and our national almanac. The ephemeris of the sun for each month occupies two pages; the first containing the equation of time; the right ascension; the declination; the sidereal time taken by the sun's *diameter* in passing the meridian, and the log. μ of the double daily variation in declination, used in determining the time from observed solar altitudes; all for *true* Berlin time, and to the same degree of accuracy as in the *Nautical Almanac*. The second page contains, sidereal time, the longitude, the latitude, the log. radius vector, and the semidiameter, for *mean* time.

Of the moon we have four pages monthly, containing her longitude, latitude, right ascension and declination, horizontal parallax, and semidiameter, for noon and midnight; and the mean time of the moon's culmination, her right ascension and declination when on the upper and lower meridian; and lastly, the times of the rising and setting of the sun and moon; the lunations, and the times of the apogee and perigee, are given at the bottom of the pages. After the whole year's ephemerides for the sun and moon are gone through, those for the planets follow next. The heliocentric longitude and latitude, the log. radius vector, the time of rising and setting, the geocentric right ascension and declination, log. of distance from earth, and the time of the culmination, are given for Mercury and Venus for every second day of the year, for Mars for every fourth day. Of the four new planets, we have only the geocentric right ascension and declination, log. of the distances from the earth and sun, the time of the passage of the meridian and the semi-arc above the horizon, for the same intervals of every fourth day,—but the right ascension, declination, and logs. of the distances, for every day during the month or five weeks about the time of the opposition, in a separate page. Of Jupiter, Saturn, and Uranus, we find the same data as for Mercury and Venus, but only for every fourth day.

The tables of eclipses, occultations, &c. of Jupiter's satellites, are very complete, for every second day of the year for the first, every third or fourth day for the second, every seventh for the third, and every fourteenth day for the fourth satellite. These are followed by,

The elements for calculating the apparent axes of Saturn's ring, for ten equidistant periods:

The apparent mean places of the principal fixed stars, according to Professor Bessel, for the year, with the formulæ, arguments, and constants for their reduction.

The catalogue of phenomena is followed by the table of occultations of planets and fixed stars by the moon, with the requisite elements for their calculation. A catalogue of stars near the moon's path for the year, with their magnitudes, right ascensions, hourly motions, and declinations, closes the almanac.

It should be borne in mind that that the Berlin ephemeris is purely an astronomical one, and is not intended for persons who are not themselves competent to supply those *arguments*, &c. which must be furnished in an almanac intended for nautical purposes; it should not, therefore, be compared with the *Nautical Almanac*, or with the *Connaissance des Tems; but, admitting this* limitation of its aim, we believe it will rank as high as either of those works for its accuracy and other merits, and it has that of having set an example, to which the attention of astronomers being called in 1830, the great improvements in our national ephemeris which we have recorded were the result.

Next to the *Nautical Almanac*, the best *astronomical* ephemeris published in this country is that which goes by the name of *White's*[*] *Celestial Atlas*, which, for upwards of thirty years, to our personal knowledge, and for much longer, we believe, has maintained its high character for utility and accuracy, and still presents itself to us with its well-known, familiar title-page, and its Greek name in red letters at the head, while the alternate repetition of lines of the same hue, remind us of former times in typography, and, by powerful association, link its present modernised contents with the bye-gone days of our youth, when we referred to its respected pages, when we drank our first draught of lore in that science which is above all price, for imparting the best kind of knowledge,—that of man's insignificance. On opening the number for 1837, we find that same neat, economical arrangement of its pages, by which the greatest quantity of matter possible is got into a given space, without confusion, that has characterised it since we can remember it. *White's Ephemeris* is the especial favourite of all *small* amateurs of astronomy, who may possess a *small* equatorial, a *small* "achromatic Dollond," a pocket chronometer by the "elder Arnold," and a *small* pair of Carey's old globes, in a *small* attic observatory; whither they love to retire, when all their neighbours are going to bed; with serenity in their hearts, and *White* in their hand.——And here we must leave them, and arouse from the "sweet and bitter fancies," stirred by the remembrance of old friends, now at rest, and of the pursuits of former days, to give our readers an account of the valuable astronomical matter to be found in *White's Ephemeris*.

We have, in the two 12mo. pages devoted to each month, the day of the month and year, and the saints' days, &c. still religiously preserved as "red letter days." The rising and setting of the sun and moon, the equation of time, the sun's declination and longitude. The moon's declination, and time of passing the meridian, her horizontal parallax, and her longitude and latitude; and the right ascension of the five principal planets, given to the nearest second in most instances, or, if this be not necessary, to the nearest minute, for *every day* of the month. The sun's semidiameter, and the time of its semidiameter passing the meridian; the time of beginning of daylight; the increase of the day, and its length; the sun's hourly motion; the log. of the radius vector; the place of the moon's node; the geocentric and heliocentric longitudes of the five planets; their declination, and their times of rising; all for every sixth day of the month; and, lastly, the lunations. All these are taken with care from the *Nautical Almanac*, or calculated from the data furnished by that work, where the results are not directly given in it.

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[*] There was an almanac, named White's, published as early as 1624; we do not know whether the present White's is a descendant from this old one, or not; that is, *whether* it inherits its title; but if so, it is not in an uninterrupted line, since the present number for 1837, only professes to be the 88th, which would fix its origin in 1749.

After the monthly ephemerides we find those of the "new planets," including that of Uranus, which still ranks as juvenile, in the opinion of the antiquated Mr. White, and as not having yet attained, by the sedateness and decorum of his conduct, the privilege of taking his place with the *dii majores.*

The right ascension, declination, and time of culmination of these five planets is given, for every sixth day for Uranus, and for every fourth for the others; and for every day during the month in which their opposition takes place; on the day of which phenomenon the data are interpolated for midnight also. To these data the logarithm of the distance from the earth is added for every eighth day.

Then follows a tide table for "London new Bridge" for every day in the year, with data for finding that at several of the more important ports, &c. on the coast.

The eclipses of Jupiter's satellites, which are *visible* at Greenwich, are given from the *Nautical Almanac* to the nearest second for mean time (which we need hardly mention is that used throughout the ephemeris, after the laudable example set by the *Nautical Almanac* since 1833).

In the "Speculum Phænomenorum," (we love these *learned* titles in barbarous Latin, in almanacs, where they only ornament useful truth, and do not cloak knavery and folly,) all the "phenomena" of the *Nautical Almanac* are found, with the addition of the apparent axes of Saturn's ring, &c. the mean time of the occurrence being given, instead of the less direct data of the original.

This is followed by a table of the sun's right ascension, in sidereal time, at mean noon, for Greenwich, for every day in the year. A table of the occultations of fixed stars by the moon, and that of the illuminated portion of the disks of Venus and Mars, from the *Nautical Almanac;* and lastly, "*Bishop*" Brinkley's catalogue of 40 stars.

The appendix contains tables for correcting altitudes for dip of the horizon; of semi-diurnal arcs for every degree of latitude to 65°, and of declination to 36°; for reducing the sun's declination to any given meridian, and to any time under that meridian; for correcting the time of the moon's passage of the meridian; a table of the hour-angle, and altitude of the sun when due east or west, for every degree of latitude to 70, and for every degree of declination from 3° to 23° 28′; a table of amplitudes equally comprehensive; one for computing equations to equal altitudes; a table of the length of a second of longitude and latitude, for every degree of the latter to 69°; a table for correcting barometrical observations for altitudes of places; and lastly, the radii of the visible horizon for different altitudes above the level of the sea.

Besides the matters we have above enumerated, there are many formulæ and observations on the use, &c. of the various articles of the ephemeris, introduced wherever the smallest vacant space afforded an opportunity, bearing testimony to the able and zealous manner in which the editor, Dr. Olinthus Gregory, devotes his talents to this useful and honourable task*.

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* This ephemeris consists of 70 pages of neat, closely-printed tabular matter, and is sold for one shilling and sixpence. We think it would be advisable to adopt, in this publication, the plan of abolishing the use of signs, and of giving the degrees

It would be foreign to the object of this paper, and to the character of our magazine, as well as far beyond our limits, to notice the host of almanacs, in the most popular sense of the term, that is, of annual publications containing the most generally useful data respecting the motions, relative distances, and other phenomena of the heavenly bodies, combined with the articles of the calendar, and with a variety of heterogeneous matter; generally, with respect to the former part, none of these works sufficiently explain to their readers the simple fact, that no almanac can be absolutely true for more than one spot, and that the same cannot equally serve for the Land's End, John o'Groat's House, and the westernmost point of Ireland. A popular almanac should, therefore, not only contain those articles which may be useful to the majority, but should furnish distinct rules, illustrated by examples, of the modes in which its data may be adapted to any other place within the probable circuit of its sale; and should furnish the tables necessary for making these corrections. Thus, the times of the rising and setting of the sun and moon, calculated for the latitude of Greenwich, will clearly not suit that of York; nor will the time of the moon's passage of the meridian be the same for Greenwich and Bristol; facts of which three-fourths of the buyers of almanacs are ignorant.

The *British Almanac* ought, however, to be cited, for its laudable and effectual attempts to accomplish much of this, as well as for the quantity, variety, and utility of the popularly scientific, as well as miscellaneous information it contains; the addition of meteorological mean data for each month, the table of the duration of moon-light throughout the year, the auxiliary table for finding the time of the sun's rising and setting, the floral calendar, and the instructions for what ought to be done in the garden, the Hebrew and Mohammedan calendars, and many other equally curious and valuable matters, may be instanced, as peculiarly proper for such a popular publication, and certainly as placing it at the head of them all.

When the French *Bureau des Longitudes* was established, one of the articles of the *ordonnance* directed the publication of an *Annuaire*, for popular use, under the immediate superintendence of the scientific men constituting the board; this *Annuaire* being a sort of feudal rent for their tenures, voluntarily undertaken, and ever since most scrupulously paid. The first of these *Annuaires* was published in 1798.

Their contents, besides the usual information of every kind contained in a popular astronomical almanac, consist of extensive and useful tables, on the various weights, measures, and coins, ancient and modern, of different states, with the reduction of those which are obsolete in France into the new *metrical* system; comparisons between the English measures, and those of other countries, with the French; statistical details as to mortality throughout all the departments of the king-

continuously up to 360, as is done in the *Nautical Almanac*; and this is literally the only improvement of which the work seems to us to be capable. But we cannot refrain from expressing our surprise and regret, that Dr. Gregory should allow such a notice as that which appears at the bottom of p. 48, in any work bearing his name. If the "company" wish to make the most respectable of their publications a vehicle for a puff of their most disgraceful ones, Dr. Gregory should not sanction the proceeding. Who may his *astrological friends* be?

dom; heights of mountains; specific gravities of bodies; dilatations of various bodies by heat, &c. &c.

But that which peculiarly characterises this admirable publication is the appearance, each year, of one or more scientific notices voluntarily contributed by M. Arago, one of the members of the board: their nature and purport may be best appreciated, by an enumeration of the subjects treated in them from 1829 to 1836 inclusive.

On the steam engine, in the *Annuaire* for 1829.

On the bursting of the boilers of steam engines.

On the relative antiquity of the different mountain-chains of Europe, in that for 1830.

On the polarization of light, and on interferences.

On light-houses, in that for 1831.

On comets. This celebrated paper has been translated, and published separately, by Lieutenant-Colonel Gould; it is by far the most comprehensive and valuable popular treatise on the subject that has ever been written.

On the influence of the moon on our atmosphere, as affecting the weather, and on her influence on organic creation.

On the congelation of running water, and the accumulation of ice in streams, in the *Annuaire* for 1833.

On the heat of the globe.

On multiple stars.

On the voltaic pile, in that for 1834.

On Artesian wells, in 1835.

Questions for solution relating to Meteorology, &c. in the number for 1836. A translation of this memoir is now being given in our pages, as our readers are aware.

These various subjects are treated by M. Arago in a manner that places him as high among popular instructors, as he stands among scientific philosophers; the superiority of the French, in the art of teaching, must be admitted by all candid persons: there are few sciences or arts on which such complete, useful, and intelligible elementary works are written, as in French. The principal reasons for this superiority we take to be, that the first *savants* of that country are sincerely anxious to disseminate knowledge among their countrymen; that they are not too proud, nor too idle, to address themselves to the task, with every zeal for its successful accomplishment; nor too mercenary to devote the requisite time, without wish or expectation of adequate remuneration; and lastly, secure of their well-earned reputation for profound knowledge, they are not vain enough to be always displaying it, even when instructing the young. They have studied the philosophy of education most successfully, to judge by the results; for, though M. Arago ranks high among them, he has many equals, if not superiors, in that most valuable of all arts,—the art of instructing*.

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* In support of our opinion, we would cite the following elementary works, at random, on various sciences:—

Francœur on Pure Mathematics.
Biot, Treatise on Physics.
La Place, System of the World.
Cuvier, Theory of the Earth.
La Vallée, Descriptive Geometry.
Dupin, on Geometry applied to the Arts.
Bourcharlat on the Differential Calculus, &c. &c.

Such being the model, it may well excite surprise, that it was not imitated in England till the present time; and what is singular, we now owe the attempt to a French bookseller (M. Baillière) settled in London. Not a year has passed since the commencement of this century, without the appearance of one or more new almanacs; but all have been professedly framed with a view to *utility** to some one class of the public: we have Merchants' almanacs, Housekeepers' almanacs, University almanacs, &c. but as yet, no English publisher seems to have recognised the existence of a body of scientific men, who, pursuing knowledge for her own sake independently of worldly profit, may yet have required a portable almanac, with a quantum of miscellaneous matter, more immediately connected with their studies; not consisting of tables of interest, transfer days at the bank, holidays at the public offices, bankers of London, lists of the houses of lords and commons, and of mail coaches, and so on; but precisely of such tables as constitute the bulk of those in the *Annuaire*, a work, by the bye, intended for, and used by, all classes in France.

Adopting the idea from the French work, a "British Annual" *should* have been a *paraphrase*, and not a *translation*; the tables should have been as much framed for the moral and physical latitude and longitude of London, as the astronomical almanac is calculated for the mean time and meridian of our national observatory. This, however, we regret to say, has not been the view of the matter taken by the editor of the *British Annual;* he has contented himself with *translating* the tables from the *Annuaire*, only converting the measures and weights to the English standards; causing a most amusing incongruity between the professed, or ostensible, and the real pervading spirit of the work. Thus, among innumerable instances of this, we may cite the "heights of some buildings," p. 42, in which there are those of four Parisian edifices, and only that of St. Paul's in London. We have the height of the column in the Place Vendôme, but *not* that of our Monument; we have the altitude of the platform of the royal observatory at *Paris*, but neither the height above the mean level of the Thames of the hill in Greenwich Park, nor of the rain-gauge at Somerset House; the height of the tower of Notre Dame may interest an Englishman, but he would like to have been able to have compared it with that of Westminster Abbey, which he *cannot* from the *British Annual;* and lastly, we have given us the height of the mast of a *French* 120-gun ship, instead of that of the Regent, Victory, or Prince. In the table of the heights of some inhabited places, we have, naturally enough in a French *Annuaire*, those of four villages in the French Alps, the Paris observatory (first story), &c. but not one of the villages among our mountainous regions, such as they are, which ought to have been found in a British work of reference.

The haste and inattention with which the whole work has been got up, manifests itself in every page, in most amusing ambiguities and blunders. We have *Anvers* retained instead of Antwerp, *Malines* instead of Mechlin, *Mayence* instead of Mentz; and innumerable other mistakes in orthography betray the *French* original, through the unfortunate incapacity for spelling foreign names rightly, which distinguishes our neighbours.

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* This word, in England, always implies, the art of getting and saving money.

In the tables for the reduction of heights obtained by barometrical observations, we find the *centigrade* thermometer used, instead of Fahrenheit's, which renders these tables comparatively useless to an Englishman. In the table of the latitudes and longitudes of the principal observatories, which has been taken from that in the *Nautical Almanac* for 1837, or from the *Annuaire* of Brussels, by M. Quetelet, the names of the astronomers at some of the most important have been retained, and inserted between brackets; but, as the initial of their titles of M., Dr., Prof., &c. is frequently omitted in the copy, though always given in the original, the reader is exposed to the liability of confounding the name of a locality for that of an observer, and would naturally suppose the word (Borgenhausen), as it is wrongly spelt*, was the appellation of the astronomer at Munich, instead of its being that of the suburb where the observatory is situated; and if, in this instance, he detected his error, and determined to avoid a repetition of it, he would infer that (Cacciatore), (Flaugergues), &c. were localities at Palermo, Viviers, &c. and not, as they really are, the names of celebrated astronomers; moreover, the longitudes ought either to have been given, both in degrees and in time, or if in only one of the two, the former should certainly, under these circumstances, have been preferred.

But we observe a more serious error in the table of the surface and population of the globe, p. 151; this is, as usual, literally *translated* from Quetelet's work, and *milles carrés* is rendered *square miles*, which always means our *statute* miles, while those used by Balbi in the original table are *geographical* miles. The numbers in the *British Annual* are hence all too small; and to complete the matter, the summary at the end of the table is even grossly at variance with the preceding details.

In the almanac part, the plan of the *Annuaire* has been injudiciously departed from. In that work, the successive columns for each month contain the sun's rising, setting, and its declination; the equation of time; the moon's age, her passage of the meridian, her rising, her setting, and those of the six principal planets, with the time of their culmination: all this is the kind of astronomical information which is expected in a popular almanac; and the substitution of the sun's semidiameter for its rising and setting is anything but an useful alteration, whatever the insertion of two columns of the time of high water at London may be. A better arrangement would, however, have allowed of both being combined, and such an addition is no more than might be reasonably expected by the public, considering the enormous difference in price between the French and English works, the latter being four times as much as the former; the French *Annuaire*, consisting on an average of 266 pages, being sold for one franc (in England for one shilling), while the *British Annual*, of 375 widely and wastefully printed pages, is priced *three shillings and sixpence*.

The account of the universities, and of the emoluments of the professors, given in the new annual, would be both interesting and valuable, if it were more complete; but, for a very inadequate reason, no account is given of the English universities of Oxford and Cambridge, nor of University and King's Colleges, London; while particulars are entered into of *all* the Scottish. The statements regarding the continental institutions for education are equally partial and limited; we have no allusion made to Jena, nor Göttingen, nor to any Italian nor Spanish university, while the comparatively unimportant ones of Griefswald and Breslau are mentioned, we suppose, because they are included in the work on the state of education in Prussia which has been copied from. Nevertheless, we look on this as the most instructive part of the additions to the plan of the *Annuaire*, and we will cite two facts from the statements, presuming on their general accuracy.

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* The word should be Bogenhausen, as it is in the *Nautical Almanac*. Tübingen is also wrongly spelt Tubinguen, after the French manner; Buda is disguised as Bude; Gœttingen, *a la Françoise*, is not countenanced by Königsberg, which is the right mode of writing the German diphthong.

In the Scottish universities, supposing that the incomes of the professors may be assumed as indicating the degree of estimation in which the different branches of study are held, it seems, from the tables before us, that these will rank as follows:—Latin (humanity!); Greek; materia medica, practice of physic, anatomy and surgery, and the other departments connected with the medical profession; then the various studies of law; while mathematics, physics, natural and moral philosophy, logic, and history, hold very subordinate places.

As a *pendant* to this statement, we will extract an account of the proficiency expected in the *candidates for admission* to the *Ecole Polytechnique* in France. They are previously examined in arithmetic, logarithms, elements of geometry, conic sections and geometrical analysis, algebra, as far as equations of the second degree, the solution of indeterminate problems, and the resolution of equations by approximation, the use of logarithms, sines, &c. in plane and spherical trigonometry, and the elements of mechanics, as far as the composition and resolution of forces, and their equilibrium in simple machines: the candidates are also expected to translate a portion of a Latin classic, to write a *theme* in their native language on a given subject, and to copy a drawing, partly shaded, in chalk*. Thus a degree of knowledge is expected from French boys of sixteen, *commencing* their education in this establishment, which is not possessed by nine out of ten English ones of twenty, on *finishing* theirs, in private and ordinary schools; and not by two out of five, on quitting Cambridge or Oxford.

In following out the plan of the *Annuaire*, Dr. R. D. Thomson has presented us with five original papers; the first, on the recent progress in optical science, by the Rev. B. Powell, of Oxford; experiments and observations on visible vibration and nodal divisions, by Mr. C. Tomlinson, of Salisbury; on the recent progress of astronomy, by Mr. Woolhouse, of the *Nautical Almanac* establishment; the commencement of a history of the magnet, by Mr. Davies, of the Woolwich Academy; and a discourse on the recent progress of vegetable chemistry, by the editor. We have not room to enter into any consideration of these papers, except to notice the singular disproportion in the space allotted to them. In an *almanac*, we have the progress of *astronomy* dismissed in ten pages, while ninety are occupied by a disquisition on the early history of the loadstone!

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* It is stated in the *Annual*, that "the number which underwent these examinations, and were admitted as pupils, in 1835, was 132."

A POPULAR COURSE OF GEOLOGY.

IV.

Materials of which Rocks are composed.

Although it is not necessary for a geologist to be profoundly versed in chemistry and mineralogy, yet a certain general acquaintance with those sciences is indispensable to the successful prosecution of his own. It will therefore be our endeavour, in the present number, to communicate to the geological student something of this necessary chemical and mineralogical knowledge.

The chemist divides all material substances into two classes,—simple and compound. Simple, or elementary substances, are those out of which nothing different from themselves can be obtained; they contain but one kind of ponderable matter. Compound bodies, are those which contain two or more elements. We are at present acquainted with but fifty-three bodies to which the title of elementary can be accorded, and out of their combinations arises all that variety of known substance existing in the earth. The list of simple bodies is liable to continual fluctuations, occasioned by the advance of chemical science. Many simple bodies have been discovered within the last thirty years, and more may be expected to be discovered. On the other hand, many supposed elementary substances have, during the same period, been proved to be compounds; and, even now, the chemist entertains suspicions of the compound nature of some bodies, which have hitherto resisted all the resources of chemical analysis to decompose them. These fifty-three bodies are called the simple ponderables;—simple, because we are at present unable to reduce them to more simple forms; ponderable, because, even in their most attenuated state, they possess sensible weight. Hydrogen, which is perfectly invisible, and the lightest substance known, may be confined in proper vessels, and the weight of even a cubic inch of it may be detected by delicate balances. If light, heat, and electricity, be really material substances, and not rather the effects of certain motions, or affections, common to matter, they are wholly destitute of weight, or, at least, if they possess any, we are unable to discover it, by the most nicely-constructed balances. And yet they appear to be acted on by some of the same powers, under some of the same laws, to which matter is subject, and the phenomena to which they give rise may be explained on the supposition of their being extremely subtle forms of matter. On this supposition, they have received the name of the *imponderables*.

Matter presents itself to our observation under three forms,—the *solid*, the *fluid*, and the *gaseous*, or aëriform. Solids, are bodies whose particles are prevented, by what is called the *attraction of cohesion*, from moving freely among themselves. Fluids, are bodies whose component particles are free to move upon one another, with very slight friction. Aëriform bodies, are lighter than solids and fluids, and their particles appear to be wholly destitute of cohesion. Such bodies are called elastic fluids, because they yield readily to pressure, and expand when the pressure is removed, Non-elastic fluids do not yield perceptibly to ordinary pressure, nor do they dilate perceptibly on its removal.

Solids may be converted into fluids, and fluids into gaseous bodies, by the agency of heat,—as when ice or lead is melted, or mercury converted into vapour, or water into steam; and different substances require different temperatures for the production of these changes. Aëriform bodies which can be condensed by cold and pressure into fluids, are called *vapours;* others are permanently elastic, and by no degree of cold or pressure can they be converted into a less attenuated form. To these only, is the term *gases* properly applied. Oxygen, and hydrogen, of which we shall speak presently, are gases. Chlorine, at ordinary temperatures, is a vapour, which may be compressed into a fluid; and water, which at ordinary temperatures is a fluid, is converted, by the addition of heat, into a vapour.

The fifty-three elementary bodies, at present known, may be conveniently classed under the following heads:

Five gases, or vapours:—Oxygen, hydrogen, nitrogen, chlorine, and fluorine.

Seven non-metallic solids and fluids:—Sulphur, phosphorus, selenium, iodine, bromine, boron, and carbon.

Three metallic bases of the alkalies:—Potassium, sodium, and lithium.

Four metallic bases of the alkaline earths:—Barium, strontium, calcium, and magnesium.

Five metallic bases of the earths:—Aluminum, silicum, yttrium, glucinum, and zirconium.

Twenty-nine metals, whose combinations with oxygen produce neither alkalies nor earths:—

1. Manganese,
2. Iron,
3. Zinc,
4. Tin,
5. Cadmium,
6. Arsenic,
7. Molybdenum,
8. Chromium,
9. Tungstenum,
10. Antimony,
11. Uranium,
12. Columbium,
13. Nickel,
14. Cobalt,
15. Cerium,
16. Titanium,
17. Bismuth,
18. Copper,
19. Tellurium,
20. Lead,
21. Mercury,
22. Silver,
23. Gold,
24. Platinum,
25. Palladium,
26. Rhodium,
27. Osmium,
28. Iridium,
29. Vanadium.

Of these metals, the first five decompose water at a red heat. The next fifteen do not decompose water at any temperature, and their oxides are not reduced to the metallic state by the sole action of heat. The oxides of the rest are decomposed by a red heat.

A glance at the above list of elementary bodies is sufficient to satisfy us that the larger proportion of them are of a metallic nature. Metals are distinguished by the following characters:—they are conductors of electricity, and of caloric, or heat. When their combinations with oxygen, chlorine, sulphur, or other similar substances, are decomposed by the action of the galvanic battery, the metals always appear at the *negative* pole, and hence are said to be *positive* electrics. They are opaque, not permitting the passage of light, even when reduced to thin leaves. They are, for the most part, good reflectors of light, and have the well-known peculiar lustre, called the metallic.

All the elementary bodies* enter into the composition of that great variety of minerals and rocks of which the crust of the earth is composed; some of them occurring in the greatest abundance, others being exceedingly rare. Those which are most abundant, are usually found in combination, seldom in an elementary form. Oxygen, the most abundant of all, does not occur in nature except in combination; and palladium, one of the most rare, is only met with in the metallic state. Few of the elementary bodies are ever found uncombined. The following are all that are ever so found, and then only in small quantities:—Carbon occurs pure, only in the diamond. Native, or pure sulphur, is exhaled from volcanoes. Gold, silver, palladium, platinum, mercury, copper, iron, antimony, bismuth, and arsenic, are the only metals that have been found in the metallic state. Notwithstanding the tendency to combination which subsists among the elementary bodies, the substances produced by their combinations are neither so complex, nor so numerous, as might be supposed. Few minerals contain more than five or six elementary constituents, and many contain only two or three. If, indeed, the affinity, or disposition to combine, were equally great between every two of the simple bodies, there might, of course, be as many minerals as there are possible combinations between fifty-three different substances; and if they combined in all proportions, the number of minerals would be endless; but the affinities vary in degree, so that those substances between which the strongest affinities subsist combine, and thus prevent combinations between those possessing more feeble affinities; and some of the elementary bodies have no affinity for each other, and cannot, by any means, be made to combine. Thus, the affinity of the metallic bases of the alkalies for oxygen is so great, that it is with the utmost difficulty they can be separated from it; or, when separated, preserved long in that state. Gold and platinum, on the other hand, have so weak an affinity for oxygen, that the ores of those metals never occur as oxides.

There is, moreover, another important law of chemical combination, which exercises a powerful influence in limiting the number of compound bodies,—namely, that in all bodies which are not mere mechanical mixtures, but chemical combinations, the ingredients of which they are composed always unite in definite and invariable proportions.

Thus, hydrogen, which is the lightest of all bodies, combines with eight times its weight of oxygen to form water; and this is the lowest proportion in which oxygen enters into combination. Taking, then, hydrogen as unity, the combining proportion of oxygen will be 8, and the combining proportion of water will be 8 + 1, or 9. The combining proportion of bodies is sometimes called their *atomic weight*, or their *representative number*, or *equivalent*. Again, oxygen and calcium combine in no other proportion than, oxygen 8 parts† + calcium 20 parts = 28 oxide of calcium, or lime. When a body combines with another in more than one proportion, the proportion in each successive compound is an even multiple of the lowest combining proportion. Thus sulphur combines with oxygen in two proportions:—

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* With one exception, for iodine has not yet been found in any mineral.

† Parts always mean parts by weight.

1. Sulphur 16 parts + oxygen 8 parts = 24 sulphurous acid.
2. Sulphur 16 parts + oxygen 16 parts = 32 sulphuric acid.

Nitrogen and hydrogen afford another exemplification of this law of chemical combination. They form five different compounds:—

1. Nitrogen 14 parts + oxygen 8 parts = 22 nitrous oxide.
2. ——— 14 ——— 16 = 30 nitric oxide.
3. ——— 14 ——— 24 = 38 hyponitrous acid.
4. ——— 14 ——— 32 = 46 nitrous acid.
5. ——— 14 ——— 40 = 55 nitric acid.

Binary compounds are substances composed of two elementary bodies united in definite proportions, which are well known and invariable. The binary compounds of most frequent occurrence in the mineral kingdom are, alkalies, earths, metallic oxides, alloys or combinations of metals in their metallic state, acids, sulphurets or combinations of bodies with sulphur, and carburets or compounds of bodies with carbon; and when these binary compounds unite, they unite, like the simple bodies, in definite proportions. Lime is a binary compound, consisting of calcium 20 parts + oxygen 8 parts = 28 lime, or oxide of calcium. Carbonic acid is likewise a binary compound, consisting of carbon 6 + oxygen 16 (two proportions) = 22 carbonic acid, and these numbers 28 and 22 are the proportions in which these two binary compounds, lime and carbonic acid, unite to form carbonate of lime; in every 50 grains of which there are 28 of lime and 22 of carbonic acid. Now, though carbonate of lime is a compound of two binary compounds, it contains but three elementary substances, calcium, oxygen, and carbon. It is, therefore, called a ternary compound. This is a very extensive and important class of bodies, comprehending most of those substances termed salts. It is necessary in this place to say something respecting the modern system of chemical nomenclature; and Dr. Turner's explanation of it is so clear and elegant, and at the same time so concise, that it would be unpardonable either to attempt an abridgment of it, or to offer any sketch of our own. "Chemistry," he says, "is indebted for its nomenclature to the labours of four celebrated chemists, Lavoisier, Berthollet, Guyton-Morveau, and Fourcroy. The principles which guided them in its construction are exceedingly simple and ingenious. The known elementary substances, and the more familiar compound ones, were allowed to retain the appellations which general custom had assigned them. The newly-discovered elements were named after some striking property. Thus, as it was supposed that acidity was always owing to the presence of the 'vital air' discovered by Priestley and Scheele, they gave it the name of *oxygen*, derived from the Greek words signifying generator of acid; and they called 'inflammable air' *hydrogen*, from the circumstance of its entering into the composition of water.

"Compounds of which oxygen forms a part, were called *acids*, or *oxides*, accordingly as they do or do not possess acidity. An oxide of iron, or copper, signifies a combination of those metals with oxygen which has no acid properties. The name of an acid was derived from the substance acidified by the oxygen, to which was added the termination *ic*. Thus, sulphur*ic* and carbon*ic* acids, signify acid compounds of sulphur and carbon with oxygen gas. If sulphur, or any other body, should form two acids, that which contains the least quantity of oxygen is made to terminate in *ous*, as sulphur*ous* acid. The termination in *uret* was intended to denote combinations of the simple non-metallic substances, either with one another, with a metal, or with a metallic oxide. Sulph*uret* and carb*uret* of iron, for example, signify compounds of sulphur and carbon with iron. The different oxides or sulphurets of the same substance, were distinguished from one another by some epithet which was commonly derived from the colour of the compound, such as the black and red oxides of iron, the black and red sulphurets of mercury. Though this practice is still continued occasionally, it is now more customary to distinguish degrees of oxidation by derivatives from the Greek. *Prot*oxide signifies the first degree of oxidation, *deut*oxide the second, *trit*oxide the third, and *per*oxide the highest. The sulphurets, carburets, &c. of the same substances are designated in a similar way. The combinations of acids with alkalies, earths, or metallic oxides, were termed *salts*, the names of which were so contrived as to indicate the substances contained in them. If the acidified substance contains a maximum of oxygen, the name of the salt terminates in *ate;* if a minimum, the termination *ite* is employed. Thus sulph*ate*, phosph*ate*, and arseni*ate* of potash, are salts of sulphur*ic*, phosphor*ic*, and arsen*ic* acids; while the terms sulph*ite*, phosph*ite*, and arsen*ite* of potash, denote combinations of that alkali with the sulphur*ous*, phosphor*ous*, and arseni*ous* acids. The advantages of a nomenclature which disposes the different parts of a science in so systematic an order, and gives such powerful assistance to the memory, is incalculable. The principle has been acknowledged in all countries where chemical science is cultivated; and its minutest details have been adopted in Britain. It must be admitted, indeed, that in some respects the nomenclature is defective. The erroneous idea of oxygen being the general acidifying principle, has exercised an injurious influence over the whole structure. It would have been convenient also to have had a different name for hydrogen. But it is now too late to attempt a change; for the confusion attending such an innovation would more than counterbalance its advantages. The original nomenclature, therefore, has been preserved, and such additions have been made to it as the progress of the science rendered necessary. The most essential improvement has been suggested by the discovery of the laws of chemical combination. The different salts formed of the same constituents were formerly divided into *neutral*, *super*, and *sub*-salts. They were called neutral, if the acid and alkali are in proportion for neutralizing one another; super-salts, if the acid prevails; and sub-salts, if the alkali is in excess. The name is now regulated by the atomic constitution of the salt. If it be a compound of one equivalent of the acid to one equivalent of the alkali, the generic name of the salt is employed without any other addition; but if two or more equivalents of the acid be attached to one of the base, or two or more equivalents of the base to one of the acid, a numeral is prefixed so as to indicate its composition. The two salts of sulphuric acid and potash are called sulphate and *bi*-sulphate; the first containing one equivalent of the acid to one equivalent of the alkali, and the second salt two of the former to one of the latter. The three salts of oxalic acid and potash are termed the oxalate, *bin*-oxalate, and *quadro*-oxalate of potash; because one equivalent of the alkali is united with one equivalent of acid in the first, with two in the second, and with four in the third salt. As the numerals which denote the equivalents of the acid in a super-salt are derived from the Latin language, Dr. Thomson proposes to employ the Greek numerals *dis*, *tris*, *tetrakis*, to signify the equivalents of alkali in a sub-salt. This method is in the true spirit of the original framers of our nomenclature. Chemists have already begun to apply the same principle to other compounds besides salts; and there can be no doubt that it will be applied universally, whenever our knowledge shall be in a state to admit of its introduction*".

The above account of the present system of chemical nomenclature explains the nature of the substances enumerated in our list of binary compounds, under the names of acids, oxides, sulphurets, and carburets. It will, however, be as well to describe the characteristic properties which distinguish acids and alkalies. Alkalies have a peculiar pungent taste; they neutralize acids; they change some vegetable blues to green; they change to a reddish brown the yellow colour of paper stained with turmeric; and they restore the blue colour of litmus paper reddened by the action of acids.

Acids are compounds capable of uniting in definite proportions with alkaline and earthy bases, and, when in a state of solution, they either have a sour taste, or redden litmus paper. Most acids contain oxygen as one of their elements, which was therefore supposed, at one time, to be the acidifying principle; but acids exist which have no trace of oxygen, and there are bodies (water, for instance,) which contain a large proportion of oxygen without possessing acid properties. Those acids in which hydrogen is one of the elements, are called hydracids.

In analyzing a mineral, it is sufficient if we ascertain the nature and proportions of the binary compounds of which it consists; and the results of an ultimate analysis may then be deduced from calculation. Thus having ascertained that 50 parts of carbonate of lime contain 28 parts of lime and 22 of carbonic acid, and knowing that lime consists of carbon 6 + oxygen 16 parts, we find that in 50 parts of carbonate of lime, reduced to its simplest constituents, there are 20 parts of calcium, 6 of carbon, and 24 of oxygen.

When we speak of simple minerals, we use the term simple in a different sense from that in which it is used by the chemist. He applies it only to those bodies which resist all the powers of chemical analysis to decompose them. The mineralogist, on the other hand, sees, aggregated in rocks, certain minerals distinguished from each other by peculiar characters. He can separate them from one another by mechanical means, and in this separate state he makes them his study, and calls them simple, because he cannot reduce them to a more simple state, except by means of chemical analysis.

Marble, or carbonate of lime, is, in this view, a simple mineral, because, although we reduce it to the finest possible powder, we have still nothing but fine particles of carbonate of lime, each consisting of lime and carbonic acid, and it is only by subjecting it to the action of heat or acids that we can obtain these elements in a separate form. If we expose carbonate of lime for a considerable time to a red heat, we drive off the carbonic acid, and oxide of calcium remains; the loss of weight indicating the quantity of carbonic acid expelled. Or, if, into a small flask containing diluted muriatic acid, the weight of the flask and acid being ascertained, a known quantity of carbonate of lime, reduced to small fragments, be gradually introduced (care being taken to prevent any of the liquid from being thrown out by effervescence), the diminution of weight experienced by the flask and its contents will indicate the quantity of carbonic acid which the carbonate of lime contained; the lime now remaining in solution as muriate of lime, from which it may be separated by the proper re-agents.

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* TURNER's *Elements of Chemistry*, p. 125.

Granite is a compound rock, consisting of three minerals. By slightly pounding a piece of this rock in an iron mortar we may separate these, and may proceed to examine those characters which distinguish them from each other, and from other minerals. We may determine their specific gravity, or the weight of each compared with an equal bulk of water. We may ascertain the shape of their crystals, if they occur regularly crystallized; or if only confusedly crystallized, the shape of the primitive form of their crystals,—that is, the shape into which they may be reduced by dividing them in directions parallel to the planes of natural cleavage. We may examine their hardness indicated by their yielding, or not yielding, to the nail, the knife, or the file; by their scratching glass, or giving sparks with steel. We may try whether they are brittle, tough, flexible, or elastic. We may examine their colours, and other relations to light, as opacity, transparency, translucency, and refraction. We may ascertain their lustre, and the aspect of a fresh fracture; also, whether they feel unctuous to the touch; whether they have any taste or smell, or adhere to the tongue; whether they are affected by the magnet, or, when excited by heat or friction, whether they exhibit electric properties, or appearances of phosphorescence. We are thus furnished with a list of external characters by which we may discriminate minerals. We may thus distinguish the constituents of granite from one another, and from other minerals; we may distinguish mica from talc, and from plate selenite, to which it has some general resemblance; and we may distinguish quartz from felspar, from calcareous spar, from carbonate of manganese, from fluor spar, and from sulphate of barytes, with all of which we have known it to be confounded by persons not conversant with mineralogy.

Having ascertained that granite is a compound of three minerals, quartz, felspar, and mica, we have recourse to chemical analysis, as in the case of carbonate of lime, in order to determine the elementary substances of which each of those minerals is composed. Our mode of proceeding, however, must be somewhat different, since each of these minerals contains several elementary substances, and since they are not soluble in acids. In order, therefore, to bring them into a state of solution, we take a given quantity, 50 or 100 grains for instance, of one of them, previously reduced to very fine powder, and fuse it in a platinum crucible, with three or four times its weight of alkali, which, uniting with one or more of the constituents of the mineral, effects its decomposition. The fused mass is now soluble in water, or diluted muriatic acid, and, by the application of different re-agents, its constituents are successively precipitated, separated from the solution by filtration, carefully dried and weighed, and their separate quantities being added together, ought to be equal to the weight submitted to analysis, a trifling allowance being made for unavoidable waste; and thus the composition of quartz, felspar, and mica is found to be:—

Quartz.—Almost wholly silica, combined with 2 or 3 per cent. of water.

Felspar:—	
Silica	63·74
Alumina	17·14
Lime	3·00
Potash	13·06
Loss and water	3·06
	100·00

Mica:—	
Silica	53·75
Alumina	24·62
Potash	21·35
	99·72
Loss	·28
	100·00

Thus, the province of the chemist with respect to minerals is to resolve them into their elements, and to investigate the properties of these, and the laws of their combination. The mineralogist makes the minerals themselves the objects of his study, investigating their characters, and the properties by which they are distinguished, and he thus learns to discriminate them when aggregated in rocks. It is in this, their aggregated state, as masses, and in their relations to other similar masses, that they become subjects of investigation with the geologist, who observes whether they occur stratified, unstratified, or in veins; and he endeavours to discover the agencies employed in their formation, and the epochs at which they were formed; only studying the minerals of which they are composed, and the elements of those minerals, so far as they may tend to throw light on those inquiries. In granite, for instance, he sees a crystalline, unstratified rock, composed of quartz, felspar, and mica, forming the foundation on which the stratified rocks rest, and also rising through them, and forming the summits of lofty mountains. He observes that it has frequently sent off veins into the superincumbent rocks, and seeing the nature of the changes produced in these at the point of contact with the granite, and their disturbed and inclined position in the neighbourhood of it, he is led, in speculating on the mode in which that rock was formed, to infer that it was once in a state of fusion, and that it was the agent employed in the upheavement of chains of mountains, and in placing in their inclined and elevated position beds of rock containing marine remains, which must have been formed at the bottom of the ocean, and in a horizontal position. Having observed, moreover, that rocks which, in different localities, have suffered disturbance in the vicinity of granite, belong to different geological epochs, evinced by their different groups of organic remains, he concludes that all the granite in the world was not formed at one and the same time, but that there have been ejections of it at various periods.

Such, then, are the different trains of investigation to which the same substances give rise in the hands of the chemist, the mineralogist, and the geologist. Let us now enter into the consideration of some of the elementary substances, which are of the most importance as ingredients of the crust of the earth.

At the head of this list stands oxygen, which combines with so many substances, and enters so largely into the composition of many minerals, and those the most common ingredients of rocks, that it may be said to form half of the ponderable matter of which the exterior parts of the globe consist. It constitutes 8 parts out of 16 of silica, 8 parts out of 18 of alumina, 8 parts out of 28 of lime, 8 parts out of 20 of magnesia, 8 parts out of 48 of potash, and 8 parts out of 32 of soda. It is an essential element of many acids. United with carbon, in the proportion of 16 to 6, it forms carbonic acid, which constitutes nearly half of carbonate of lime, which has been estimated as one-eighth of the crust of the globe. Oxygen forms 8 parts out of 9 of water, and enters extensively into the composition of many of the ores of manganese, tin, lead, iron, and copper; but it is never met with except in combination with some other substance.

A large portion of the atmosphere consists of oxygen, deprived of which, it loses its power of supporting animal life. Oxygen is not inflammable, but a supporter of combustion. Every substance that will burn in atmospheric air, burns with far greater brilliancy in oxygen gas. Even iron and steel undergo rapid combustion in it. Combustion is a combination of oxygen with the burning body, and the products of combustion in oxygen gas, or atmospheric air, are oxides.

Oxygen, in its elementary state, is an invisible, permanently-elastic gas, without taste or smell, a feeble refractor of light, and a non-conductor of electricity. When the compounds of oxygen are submitted to the action of a galvanic battery, the oxygen is always determined to the positive pole, and is therefore said to be negatively electric. Its combining proportion has already been stated to be 8.

Hydrogen.

Hydrogen is the lightest of all known material substances, and is taken as the standard of comparison for the specific gravity of the gases, as water is for that of solids and liquids. It is by no means an important element in the composition of rocks. It is, like oxygen, an invisible and permanently-elastic gas. It is highly inflammable; combined with sulphur it forms sulphuretted hydrogen, which, as well as hydrogen itself, is emitted from the earth in volcanic regions. Sulphuretted hydrogen, when absorbed by water, communicates to it the peculiar properties of the Harrowgate and other sulphureous springs. That species of limestone called swinestone, is supposed to derive its fetid odour when rubbed, from the presence of sulphuretted hydrogen. Combined with carbon, hydrogen forms carburetted hydrogen, which issues in large quantities from between beds of coal, and produces those fatal explosions known to miners by the name of fire-damp. Hydrogen constitutes 1 part in 9 of water. Water is contained in most minerals as an accidental ingredient, either inclosed in cavities, as in rock crystal, calcedony, and flint, or absorbed by most earthy and porous minerals; but it is also chemically combined *with many* minerals, both crystalline and massive, as an essential ingredient. These combinations with water are called hydrates; and when water forms an essential ingredient of a crystallized mineral, it is called its water of crystallization. Hydrogen is a positive electric with respect to oxygen, chlorine, and that class of bodies, but negative with respect to the metals. It is a powerful refractor of light. It soon causes death to an animal that breathes it. Its combining proportion is 1.

NITROGEN.

NITROGEN is likewise a permanently-elastic, invisible gas, without taste or smell. It is not combustible, but extinguishes burning bodies that are plunged in it. It is incapable, when respired, of supporting animal life; and yet, diluted with oxygen, in the proportion of 8 parts of oxygen to 28 of nitrogen, it forms atmospheric air, so essential to life. Air is not a chemical combination of these gases, but a mechanical mixture of them. A chemical combination of oxygen and nitrogen, in proportions which we have before stated, constitutes nitric acid, one of the most violent and poisonous of that class of bodies. Nitrogen combined with hydrogen forms ammonia, a volatile substance, which possesses all the properties of an alkali. The sulphate and muriate of ammonia are exhaled from volcanoes. Nitrogen, in the state of nitric acid, enters into the composition of nitrate of potash, which is an abundant production of the earth in various parts of France, Germany, Italy, Spain, Hungary, Persia, India, and America.

CHLORINE.

CHLORINE is a yellowish-green vapour, of an astringent taste, and disagreeable odour. It is perfectly irrespirable, exciting spasms and irritation of the throat, even when much diluted with atmospheric air. It may be condensed by cold and pressure into a yellow liquid. It has a violent action on some of the metals, which, when introduced, in the state of powder, or in fine leaves, into chlorine, are inflamed and enter into combination with it. It is known in the mineral kingdom as one of the constituents of chloride of sodium, or rock-salt. We have likewise ores of lead, mercury, and silver, which are chlorides of those metals; but it never occurs uncombined. With hydrogen it forms muriatic acid, which is one of the components of the salts called muriates. Muriate of soda, which by evaporation is converted into chloride of sodium, is the principal ingredient in sea water, which likewise contains muriate of magnesia. These two salts are also found in many saline springs. Muriate of ammonia has already been spoken of as a volcanic product. Chlorine is a negative electric; its combining proportion is 36. Its name is derived from its greenish colour (χλωρος).

FLUORINE.

THE substance to which this name has been given has never been obtained in an insulated form; and there is a difference of opinion among chemists, whether Derbyshire spar should be considered as a compound of calcium and fluorine, or of oxide of calcium and an acid of which fluorine is one of the elements. To this acid the name of fluoric was given, and it was supposed to consist of fluorine and oxygen, but the experiment of Sir Humphry Davy, since repeated by other chemists, affords strong presumption that fluor spar is a direct combination of fluorine and calcium, and that the acid produced when these two elements are separated by means of aqueous sulphuric acid, consists of fluorine united to hydrogen, derived from the water of the sulphuric acid. It has therefore received the name of hydrofluoric acid. It possesses the property of dissolving silica, and consequently of corroding glass. Fluor spar is an abundant mineral production. The hydrofluoric acid is also found in the cryolite, and a few other rare minerals. Fluorine is a negative electric. Its combining proportion is 18.

REPORT OF MAGNETICAL EXPERIMENTS,

TRIED ON BOARD AN IRON STEAM VESSEL, BY ORDER OF THE RIGHT HONOURABLE THE LORDS COMMISSIONERS OF THE ADMIRALTY. BY EDWARD J. JOHNSON, ESQ., COMMANDER, R.N., F.R.S.

THE very extensive use of iron in the construction of modern vessels, and still more recently, the formation of steam vessels entirely of that material, has rendered the compass, notwithstanding its successive improvements, little more than a piece of useless lumber: or, more properly speaking, it is become unworthy of confidence, and, consequently, where it is trusted, a deceptive and frivolous apparatus. Indeed, the compass in its rudest form, even the Chinese, or the early European, with ships built as ships were then built, was worthy of far greater confidence than the most improved compass is on board a vessel of modern construction. There is no doubt, indeed, that were the weather always clear, the compass might be advantageously dispensed with in maritime affairs; but, as during not only days but weeks of the most tempestuous weather, when not a single lunar can be taken, nor any kind of complete observation made, the vessel is driving about amongst known or unknown dangers, it cannot but be viewed as a most perilous condition, when the direction itself upon which she is sailing, is a matter of almost total uncertainty. If, indeed, she be out at sea, and far from land, she is safe, provided her strength be sufficient to ride out the storm: but every year, we are well assured that numerous cases of the heart-rending scenes of wreck and desolation would be escaped, had they the power to ascertain the course upon which they were bearing. This, however, is utterly impossible as ships are now built, by the use of the compass simply.

Mr. Barlow's correcting-plate is, on this account, one of the greatest boons conferred on modern navigation. This term, as most of our readers are aware, is something of a misnomer; since the plate, instead of correcting the error produced by the iron of the ship, *doubles* it: but we would not quarrel with names—as it is with things we have to deal. It enables us, by a very simple numerical process, to ascertain approximately the effect of the iron of the ship upon the direction of the needle, and to make allowance for it in our reckoning.

It is strange, however, to witness the apathy with which the reckless seaman, in time of security, looks upon the possible danger of a future, and not remote, period. A single hour in port would enable the master to find the effect of his iron with considerable precision: and yet this single hour he thinks it too much to give to his own and his crew's future safety. Strange infatuation!—but infatuation almost always follows close upon the heels of familiarity with danger.

We do not require to be told that Barlow's plate is not perfect. This we are as fully aware of as any one: and we do not urge implicit reliance upon it, in any sense of the word, under all possible circumstances. Still, if it enable us under all conditions to ascertain the amount of the effect of iron *approximately*, and often within very narrow limits, surely we must be determined upon rushing into danger, if we do not avail ourselves of it to the degree in which it can assist us. We are utterly opposed to the use of the compass at all, in those cases where it can be dispensed with: but as cases so perpetually occur where it is our *only* guide, and those cases precisely those of the greatest danger, it is surely worthy of the most serious attention, from every practical navigator, as well as from men of science*.

At the period when Mr. Barlow proposed his plan of the correcting-plate, he had in especial view the effect of the immense masses of iron which the guns on board men of war contained. Of course, *cæteris paribus*, the same circumstances would occur on board the smaller vessels in the merchant service, and require correction accordingly. The recent introduction of iron steamers has, however, given a new and important interest to this contrivance. These are chiefly designed for passengers, and, in some cases, more than five hundred are crowded on board a single steamer. We do not indeed, just now, know to what extent the iron steamers have been introduced: but as they have many advantages, in respect to security and convenience, over those of wood, they will most likely supersede them entirely, *provided they can be rendered as safe for the purposes of navigation by the compass*,—circumstances giving rise which must inevitably occur in all voyages of any considerable extent. The inquiry into this possibility, it was the main object of Captain Johnson's experiments to satisfactorily answer; and we proceed to give a brief analysis of them.

The Dublin Steam Navigation Company placed at the disposal of the Lords Commissioners of the Admiralty a fine new vessel, built entirely of iron, the *Garryowen*, for the purpose of investigating the effect of the vessel upon the indications of the compass, in any way that their Lordships may think proper†. They appointed Captain Johnson to make the requisite experiments; and he repaired in her to the port of Limerick,

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* In an early number we shall give a thorough examination of the principles of Barlow's correcting-plate; and endeavour to show the degree of theoretical evidence, combined with experiment, this method has for its foundation.

† WEIGHT OF IRON.

Total weight of iron, including hull, machinery, anchors, cables &c.	180	tons.
Weight of iron in hull . . .	95	,,
Do. . engines	40	,,
Do. . shafts and wheels .	12	,,
Do. . boilers	30	,,
Do. . chimney	1	10
Do. . anchors and cables	1	10

Stem, 14 feet long × 4 feet wide.
Beam, 4 in. deep, 4 in wide, bound with iron plates.

All iron used in hull and boilers is malleable.

DIMENSIONS OF VESSEL, &c.

Length on deck, 130 feet; beam 21 ft. 6 in.
Do. keel, 122 3 ; depth 11 ft. 0 in.
38 double frames a-midships, of angle iron 2 in. wide × 3 deep × $\frac{1}{2}$ in
17 single frames forward, of $3 \times 3 \times \frac{3}{8}$
22 do. . . aft, . . . $3 \times 3 \times \frac{5}{16}$
Diameter of chimney . . . 3 feet.
Height of ditto 28
Draught of water, forward 5 ft. 3 in. aft 5 ft.

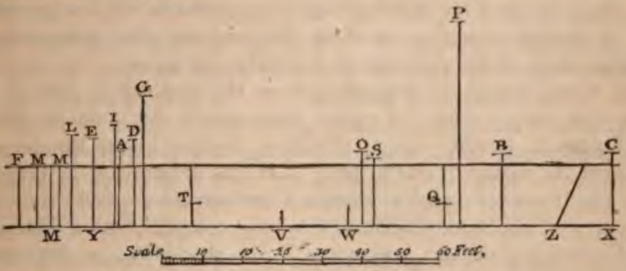
TWO ENGINES=85 HORSE POWER.

Diameter of cylinder . . 3 ft. 9 in.
Diameter of wheel . . . 15 ft. 6 in.
Engine makes 27 strokes per minute.

in the autumn of last year, to carry them into execution. The results have been printed for the Royal Society's Transactions; but they are not yet published. We avail ourselves, however, of a copy of the Memorial, with which we have been favoured, to lay before our readers a succinct account of them, and a few reflections on the results they bring to light.

There being no wet dock in the port of Limerick in which the *Garryowen* could be conveniently swung round, to make the observations in different azimuths, point by point, he fixed upon a position in Tarbert Bay, well adapted to his purpose. His operations were commenced on the 19th of October, and continued, as circumstances permitted, till the 18th of November.

In order, however, to show the positions in the vessel at which the several observations were made, the following diagram is given. The line Y Z is the keel, Y being the stern, and Z the stem; V is the place of the chimney, and W the axle of the paddle-wheel. The positions of the other points will be easily judged of, from the accompanying scale and table: and these designate the positions of the compasses named by the several letters themselves.



A	Quarter deck, 5 feet 9 inches above the deck.
B	Forecastle, 5 feet 11 inches above the deck.
C	Bowsprit, on glass-legs.
D	On the fore-part of the temporary poop, above deck 8 feet 5¾ inches.
E	On the after-part of the temporary poop, 8 feet 5 inches from the deck.
F	On a stage level with the taffrail.
G	On a plank 4 feet below the main-gaff end, and above deck, 20 feet 5 inches.
I	On the centre of the temporary poop, above deck 13 feet 4½ inches.
L	On the poop projecting over the stern.
M M M	Three stations on the stage over the stern, level with the taffrail.
O	Between the paddle-boxes.
P	Two-thirds up the fore-topmast, above the deck 40 feet 2 inches.
Q	(On glass legs) in the fore-hold.
S	In the iron sphere a-midships, above deck 7 feet.
T	In the cabin.
All	made in the middle of the vessel, 9 feet 11 inches from each side.

After the preliminary operations of fixing stations, &c. were gone through, Captain Johnson saw it to be necessary to put all the iron which the vessel carried, in the places which it usually occupied during the voyage, such as the anchors, cables, &c. He then tried the effect of the whole in that direction of the vessel where, in the generality of cases, the deviation had been found to be a maximum, so as in some degree to guide him in the selection of a place for the principal observations,—or that, which his orders especially directed, in which to place a steering *compass, and where the* effect of the plate may be successfully tried.

With this view, the vessel's head was warped to the true magnetic east, and the deviations at several parts of her ascertained:

At B, the deviation of the marked end of the needle = 16° E.
Near the centre, and before the funnel, it was . = 26° 20′ E.
At A, when the boats' davits were out . . = 11° 40′ E.
. . . . . were in board . = 6° 20′ E.

The preceding facts, showing the influence of iron in the vicinity of the compass, may be worthy of the attention of the practical navigator: for they show, at a position not far from the binnacle, a difference of deviation of no less than 5° 20′ under the mere circumstance of swinging the quarter boats' davits in board, from their usual position where boats are hoisted up, to that place in which they would be secured in stormy weather at sea.

Though the principal object of these experiments was to determine the best position for a steering compass on board the iron steamer (which induced him to select A and B as especial places of observation), Captain Johnson tried the effect of the vessel at all the places in the table given above. We need not follow him through all the details of the precautions which he used for insuring the elimination of all foreign influences of an accidental kind, nor of those for securing good observations. A brief statement of the positions is, however, necessary.

A fixed station X* was selected on the south-west side of Tarbert Bay, a mile distant from the vessel, from which the cone of the mountain Dicomede, in the county of Clare, distant about nineteen miles, was distinctly visible. The bearing of it was determined by the compass which was afterwards used at station A on board the vessel; the magnetic meridian was also then determined by this compass, and a distant object in that meridian, on land, was noted. The theodolite, in the succeeding observations, was placed at X in the same position as the compass, thereby rendering the simultaneous observations at A and X virtually identical with those which would have been determined by one compass only. The vessel was then taut moored in the line between the station X and the cone of the mountain,—in the line of direction of which, was also a remarkable heap of stones on Kilkerran Point,—so that the vertical wire of the theodolite at X bisected these objects and the instrument on the forecastle of the vessel.

The bearings of the cone of the mountain from the positions A and B as well as the simultaneous bearings between A and X, were observed when the vessel's head was at each point, and from these bearings between A and X the deviation of the compass produced by local attraction was deduced. Of course we cannot give the tables entire, and we mention the circumstances under which the observations were made, to show that every requisite care was used to ensure correct results. We are, however, still under the necessity of giving one, referring our readers to the paper itself for the others. This contains the simultaneous observations made with nine compasses in different parts of the vessel, the bell being struck as a signal for observation. *See opposite page.*

In looking at this table, it is impossible not to be struck with the

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* *No figure is given to these descriptions; nor is any necessary, as the reader can easily sketch it from the verbal statement.*

Date.	True magnetic direction of vessel's head.	Direction of vessel's head by compass F.	Deviation at F.	Direction of vessel's head by T.	Deviation at T.	Direction of vessel's head by G.	Deviation at G.	Direction of vessel's head by A.	Deviation at A.	Direction of vessel's head by O.	Deviation at O.
Nov. 5.	North.	North.	0° 0′	N. 26° 0′ E.	26° 0′	N. 1° 0′ E.	1° 0′	N. 8° 20′ E.	8° 20′	N. 16° 30′ E.	16° 30′
Therm. 52° 1.	N.E.	N. 33 0 E.	12 0	N. 74 30 E.	29 30	N. 45 0 E.	0 0	N. 55 0 E.	10 0	N. 72 0 E.	27 0
Barom. 29·8.	East.	N. 67 0 E.	23 0	S. 59 30 E.	30 30	East.	0 0	N. 80 40 E.	9 20	S. 66 0 E.	23 0
Nov. 4.	S.E.	S. 64 30 E.	19 30	S. 30 15 E.	14 45	S. 45 0 E.	0 0	S. 39 30 E.	5 30	S. 35 0 E.	10 0
Therm. 56°	South.	S. 1 0 E.	1 0	S. 10 30 E.	10 30	S. 1 0 E.	1 0	S. 1 0 E.	1 0	S. 5 0 E.	5 0
Barom. 29·7.	S.W.	S. 65 0 W.	20 0	S. 8 30 W.	36 30	S. 45 0 W.	0 0	S. 33 0 W.	12 0	S. 26 0 W.	19 0
	West.	N. 66 30 W.	23 30	S. 35 30 W.	54 30	N. 85 0 W.	5 0	S. 80 0 W.	10 0	S. 63 15 W.	26 45
	N.W.	N. 32 0 W.	13 0	N. 30 0 W.	15 0	N. 40 0 W.	5 0	N. 38 0 W.	7 0	N. 64 0 W.	19 0
		Direction of vessel's head by Q.	Deviation at Q.	P.	at P.	B.	at B.	C.	at C.		
Nov. 5.	North.	N. 18° 0′ E.	18 0	N. 1 24 E.	1 24	N. 9 40 E.	9 40	N. 1 24 E.	1 24		
Therm. 52° 1.	N.E	N. 56 15 E.	11 15	N. 47 48 E.	2 48	N. 56 0 E.	11 0	N. 43 36 E.	1 24		
Barom. 29·8.	East.	S. 73 8 E.	16 52	S. 81 33 E.	8 27	N. 82 30 E.	7 30	East.	0 0		
Nov. 4.	S.E.	S. 36 33 E.	8 27	S. 42 12 E.	2 48	S. 41 0 E.	4 0	S. 42 12 E.	2 48		
Therm. 56°.	South.	S. 9 51 E.	9 51	South.	0 0	S. 5 0 E.	5 0	South.	0 0		
Barom. 29·7.	S.W.	S. 14 3 W.	30 57	S. 41 30 W.	3 30	S. 27 30 W.	17 30	S. 45 0 W.	0 0		
	West.	S. 59 3 W.	30 57	S. 84 23 W.	5 37	S. 73 30 W.	16 30	N. 86 30 W.	3 30		
	N.W.	N. 32 21 W.	12 39	N. 47 48 W.	2 48	N. 40 30 W.	4 30	N. 36 33 W.	8 27		

immediate effect of the iron composing the vessel and her works, upon the indications of the needle, as the ship's head is turned into different azimuths, being in some cases even more than five points. It is also evident, that in different vessels this will very materially vary, and that little assistance can be derived from experiments made in one vessel towards guiding us in judging of the influence of the iron in another of a different, or even of a similar construction,—so far as form and disposition of metal are concerned in rendering them similar. To this, however, we shall speak presently. It is true that at G, C, P, the influence is much less than at any of the other positions where the observations were made; but as these are positions at which, in actual navigation, the observations could not be conveniently made, there can be no inference in favour of the employment of iron steamers deduced from these,—however interesting they may be in reference to some questions concerning the extent of the influence of iron on the magnet, and upon the law of its decrease of force as the distance is increased.

The result of these observations, as well as his first already mentioned, was conclusive to Captain Johnson's mind, that for the purposes of navigation, his observations should be chiefly confined to the positions A and B. Of two series of such observations, the discrepancies are unaccountably great, and especially so, seeing that every possible precaution was taken to secure a sameness of circumstances under which to make the observations. Captain Johnson was not, however, so much surprised at this circumstance as we should have expected; as he had remarked that the "embarrassment in the movement of the needles" after the first series in some degree prepared him to expect it,—or, in other words, that the difference in the magnetic state of the needles themselves, induced by the first series, was so great, that their indications would be materially altered in the second. This we can easily, to a certain degree, but not to the amount which these experiments indicate, conceive; the directive force of the needle may be increased or diminished by induction we admit, and then the power of the iron remaining the same, its influence would be accordingly less or greater upon the position of the needle when out of the magnetic meridian: but still that pure iron should induce *so much permanent change* in the magnetic state of the hard steel of the needle is to us inconceivable. All experiments go to prove the reverse of this. Captain Johnson's subsequent experiments prove the reverse of this, too; for they actually show that the vessel herself was a *permanent magnet*, and not a magnet by induction, as cast-iron is generally found to be. Now the needle itself could produce no sensible effect upon the state of the larger magnet; and hence her power upon the needle would be sensibly the same in both series of experiments, whilst that of the needle to resist her influence may be varied in a great degree. Captain Johnson is led to attribute the different effects (in another series of experiments which he afterwards made with the same needles) of the head and stern of the vessel to the needles placed upon quay, to the different disposition of the quantity of iron in those two parts of her; but at the distances at which he observed these deflections, we *cannot* account for it on these principles, especially keeping in view the *very* minute influence recorded to have been exercised upon the needles at G, C, and P, in his former experiments, in the table which we have copied above.

The intensity of the magnetic force in the needle was greatly altered by the iron of the vessel; the dip was also very greatly altered. All this, of course, we should expect; but we wish the *intensity* of the needles had been observed on shore both before and after the experiments were made on board; and as nearly as possible under the same circumstances. This, we consider an unfortunate omission; but we do not blame Captain Johnson for it. The time of the year was unfavourable, affording so few days fitted for observation, and the period allotted to the whole series was much too short under such unfavourable circumstances. If we are not mistaken, other observations, too, were made which are not here published; and, possibly, when they are made public, they may throw some further light upon the phenomena. However, in this respect, we are not able really to say what those observations were, nor whether they at all bear upon this question. If they do, it was a mistake to withhold them from the scientific public on the present occasion.

The two following diagrams exhibit visually the several circumstances of these experiments; and require scarcely any description.

DIAGRAM I.

Representing the deviation of several compasses, when placed in different parts of the vessel, when her head was in the true magnetic direction of the cardinal points of the compass.

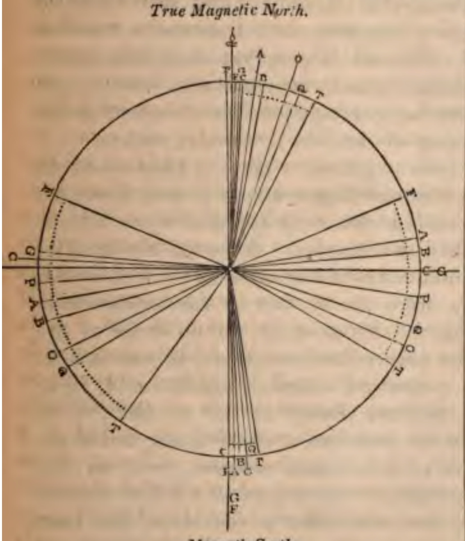
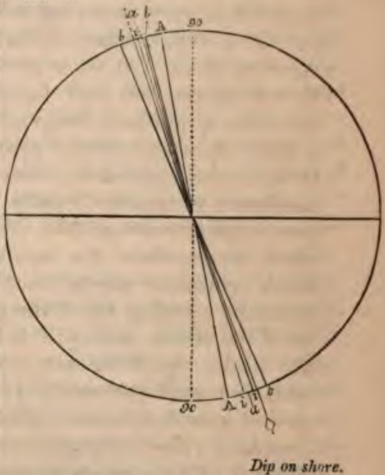


DIAGRAM II.

Representing the comparative dip of the magnetic needle, on Tarbert Island, and that observed at three positions on board the *Garryowen*, in Tarbert Bay, when the vessel's head was to the true magnetic north and south.



A, I. Dip observed at those positions with the vessel's head to the north.

a, *i*, *b*. Dip with vessel's head to the south.

When we consider the great number of parts of which a vessel is composed, and the processes by which those parts are formed, we can hardly be justified in considering the vessel as other than *an immense magazine of permanent magnets* fastened together. Were they all, indeed, so placed in building her, that their axis may be parallel, we may be better able to form some general idea of the magnetic resultant, (so to speak,) and to guess at the intensity and directive force exerted by the whole system; but, even then, the difficulties, in the present state of magnetic knowledge, would be absolutely insurmountable. When, however, we consider the effect of a single and slight stroke with a hammer upon a permanent magnetic bar—the total disregard paid in building the vessel as to the magnetic state of her materials—the utter impossibility of ascertaining it after they are put together, or to alter it, definitely, in any one of the pieces—when we consider that *malleable* iron can alone be employed, and that all these difficulties stand in the way of even placing the component parts of the compound magnet parallel to one another, we say, we are compelled to affirm, that the real magnetic state of an iron ship is incapable of being ascertained by any series of experiments whatever. Captain Johnson's experiments verify, but do not in our minds strengthen, our convictions on this head. They were formed, *à priori*, and from the view which, in common with all scientific men, we had before taken of the necessary consequences of the most familiar phenomena, as well as of the diversified experiments of a more refined class carried on by the most eminent philosophers for many years. Instead, therefore, of considering the steamer as a single permanent magnet, we ought to consider her as a vast apparatus of united magnets, distributed perfectly at random; and their relative positions and intensities altogether incapable of estimation. Are we not justified, then, in saying as we have said above, that we are incapable of judging from a series of experiments made on board one vessel, what the effect of one (so far as general form and disposition of material are concerned) similarly built in all respects may be? Surely, we are.

Let us, however, even waive this objection. Have we not seen that on board the *same vessel*, on two different days not very remote from each other, the deviation of the *same needles* was very different when all circumstances were alike, except that the magnetic states of the needles were *themselves changed* by the first series of observations? We do not, indeed know where the needles were placed during the intermediate period,—whether ashore or on board. If on shore, the influence of the short period during which the first observation was made, is only indicative of the intense action of the compound magnet, which could, in so short a time, produce such a permanent change in the needles: if on board, they were probably kept in the positions of observation, and if so, it proves how dangerous it is to trust to the *same compass*, *from one day to another*. The compass with which we leave port is a different compass after a single hour's voyage; and after three or four days, has been "deteriorated" to such a degree as to be unworthy of the slightest confidence.

When we look at the matter in this light, we need not inform our readers, that we look also upon all attempts at the correction of the local attraction, by means of Barlow's plate, as utterly useless *on board an iron steamer*. Upon this subject, however, as we said before, we intend to speak more fully in a future number: it is sufficient to say here, that Captain Johnson appears to have made but few experiments with it, owing to the "unfavourable state of the weather." It is very true that unfavourable weather is disadvantageous for good experiments for mathematical investigations of the *laws* which reign through physical phenomena; but we do really think, that, from the circumstance of the knowledge itself being only required in bad weather, the present was an advantageous opportunity thrown away; inasmuch as the amount of discrepancy to be expected in times when the compass is our only trust, is, in a practical point of view, infinitely more important than a knowledge, however accurate, of the laws which prevail when the compass is never recurred to by a properly educated sailor. It is surely of greater consequence to know how far we may *safely* trust our instruments, at the time when they are our *only* trust, than to ascertain their laws in a state of comparative repose, and when they are altogether unnecessary to our safety.

Finally, we feel it our bounden duty to our readers and to the public, to express emphatically, our conviction of the extreme danger of this class of vessels, for the purposes of navigation by means of the compass,—or, in other words, for venturing out of the mouth of a river. It is neither our wish, nor our interest, to discourage the progress of the arts and manufactures of our country; but it is both our duty as scientific journalists, and our spontaneous feeling as men, to warn our countrymen against the dangers to which they may expose themselves, by stretching beyond its due limits, the application of any product of our manufacturing ingenuity. We are fully aware of the advantages which belong to the use of iron, in the construction of iron steamers; but we also wish our readers to be fully impressed with a sense of their recklessness in daring all its dangers. We should nor fulfil our duties towards them, did we not distinctly tell them, that the iron vessel is in precisely the same condition with respect to the compass, as if no compass had ever existed,—or, in some respects, even a worse condition,—since they may be led to trust to a guide that *cannot* guide them aright, and neglect those slight glimpses of indication which may in some small degree assist them. Such, at least, is *our view* of the matter; and if we are wrong, we hope we need not say, that we shall be extremely glad to be enlightened on the subject, by those who see better than we can.

We do not think it necessary to give here Captain Johnson's determination of the best position for a sailing compass on board the *Garryowen*, as, even abating all other objections to the use of this kind of vessel, we have given good reasons why it could not be depended on for any other vessel similarly built,—much less for one in which a different disposition of materials may be adopted. On the care bestowed upon the experiments, as well as on the *evident honesty* of their record, we cannot speak too highly; and we are glad to find such men are found by the Admiralty, to be entrusted with this class of expeditions. We cannot, however, in respect to its scientific value, but regret that a more complete apparatus was not furnished to him, and that a more suitable period, both as to *date and extent, was not* selected for the purpose.

A POPULAR COURSE OF ASTRONOMY.

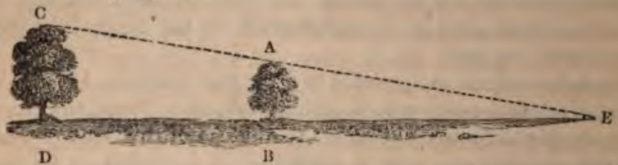
No. VII.

The Elliptical Form of the Earth's Orbit.—Kepler's Laws.

The earth revolves continually upon an axis within itself, and continually in an orbit about the sun. Hence result the alternations of day and night, the difference of the duration of the *solar* from that of the *sidereal* day, and the different times of the rising of the same fixed stars,—of which phenomena the two last constitute an *apparent* annual revolution of the sun in the heavens, inasmuch as they would result from such a revolution. The axis about which the rotatory motion of the earth takes place remains always parallel to the same line in space; and hence result the phenomena of the seasons. But all these phenomena will be equally well accounted for by a revolution of the earth round the sun, in whatever orbit that revolution may take place,—and no inquiry on which we have hitherto entered indicates with any certainty, what is the real form of the earth's orbit. Provided the earth go completely round the sun in the space of a year, it matters not, so far as the facts which have hitherto been stated are concerned, whether the motion of its centre be in a circle, in an ellipse, or in a spiral,—or, in fact, whether it be in a square or an oblong.

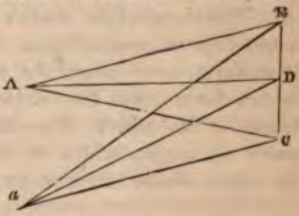
We are now about to indicate the means by which the real form of the earth's orbit is ascertained, the nature and law of its motion in that orbit, and the actual dimensions of the orbit. It will then be explained how the apparent motion of the sun, the duration of the seasons, and the length of the year, are modified by these facts.

First, then, as to the form of the earth's orbit. It is not a circle, for then the sun would at all times of the year appear of the same size to us. We judge of the dimensions of objects by the angles which lines, drawn from the extremities of them, subtend at the eye. The conclusions we thus draw, we modify, however, by that which we know of the distance of the objects. Thus two objects, AB and CD, may subtend the same angle, CED.



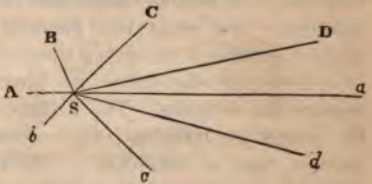
Rays of light come to us in straight lines. If, therefore, an instrument having two arms, which can be made to include any given angle between them, be placed so that one of these arms is in the direction of a ray coming from one point of the edge of the sun's disk, and the other in the direction of a ray coming from a point on the edge diametrically opposite to this, then these two arms of the instrument will include between them precisely the same angle which two lines drawn from opposite points of the sun, or opposite extremities of one of its diameters, include. Two such lines, A B and A C, will form, together with the sun's diameter B C, a triangle, of which the angle measured by the instrument will be the vertical angle B A C, and the sun's diameter the base. Now the sun's actual diameter, the base of this triangle, must be supposed to remain always the same; and if the earth moved in a circle of which the sun was the centre, the two sides A B and A C of the triangle would always remain the same whenever the observation was made, and therefore the vertical angle B A C would always remain the same; that is, the sun's apparent diameter, as measured by the instrument which has been described, would always remain the same. Now it does *not*;—the earth's motion is, therefore, not in a circle whose centre is the sun.

The sun's apparent diameter on the 31st of December 1828[*], was 32′ 35·6″, or 32·5933″; and on the 2nd of July 1829 it was 31′ 31″, or 31·5167′[†]. Now let A and *a* represent the positions of the eye of the observer on those two days. It then follows, by the rules of trigonometry, that since B A C and B *a* C are very small, B C may be considered as the arc of a circle, and that



$$\begin{aligned} BA : Ba &:: \angle BaC : BAC \\ &:: 31{\cdot}5167 : 32{\cdot}5933; \\ \text{or, } BA : Ba &:: 315167 : 325933 \\ &:: 1000 : 1034. \end{aligned}$$

Thus, if we suppose the whole distance to the sun on the 31st of December to have been divided into 1000 equal parts, on the 2nd of July its distance will have been increased by 34 of those parts. Now, between these two periods, it will be found that the sun has apparently moved through about 180 degrees of the heavens; or, in other words, that the earth has described 180 degrees about the sun: so that a line, D A, drawn from the centre of the sun to the earth on the 31st of December, and one, D *a*, drawn to it on the 2nd of July, make with one another an angle of 180°, or are in the same right line. Suppose A *a* in the accompanying figure to represent this line; take A S so as to contain 1000 equal parts, and *a* S 1034 of these parts; then, A, S, and *a* will represent the relative positions of the earth and sun at these dates. Knowing that the earth was in the position A on the 31st of December, we can tell what angle a line drawn from it to the sun in its position B at any other period, say the 1st of February, makes with S A; that is, we can find the angle A S B; in point of fact, this angle is the number of degrees measured upon the ecliptic between the sun's positions on those two days. Observing also the sun's apparent diameter, we can compare its distance on the 1st of February with that on the 31st of December; thus, taking



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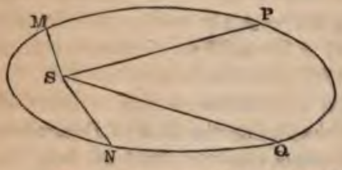
[*] *At 2h 31′ 15″* P. M.

[†] At 5h 45′ 41″ A. M.

A S as before to contain 1000 parts, we can find how many of these parts are contained by B S. Thus then we shall have determined the relative positions, A B, of the earth in respect to the sun on the 31st of December and the 1st of February,—and we may proceed similarly to ascertain the positions, C D, of the earth in respect to the sun at the commencement of each month in the year. Of these distances we shall thus discover this remarkable property, one of the laws of Kepler, that they all lie in the *circumference of a figure called an ellipse;* of which curve the characteristic property is this: that if from two given points within it, called its foci, there be drawn two lines to any point in its circumference or periphery, the *sum* of these two lines will be the same wherever that point may be situated. Of this ellipse the sun will be found to occupy one of the foci.

The form of the earth's orbit being thus ascertained to be an ellipse, a question at once arises as to the nature of its motion in that ellipse; is it uniform, as it might be supposed to be, if it revolved in a circle of which the sun was the centre? or is it in any way modified by the eccentricity of its orbit? The motion of the earth is *not* uniform; for if it were, the angles which it describes, in equal times, in different parts of its orbit, would be inversely as its distances at those times. Thus, if the earth, when at its least distance from the sun, S, or in its perihelion, described in a day an arc M N, equal to that, P Q, which it described in a day when at its greatest distance, or its aphelion, then the angle P S Q would be to the angle M S N, in the ratio of M S to S P.



Now it is ascertained by observation, that when it is in its aphelion, the earth moves in its orbit (that is, the sun moves in the ecliptic) through 57·192′ in 24 hours, and that in its perihelion it moves through 61·165′ in that time. Also its distance in aphelion we have shown to be to its distance in perihelion as 1034 to 1000; it follows, then, that if the earth's movement in its orbit were uniform, the ratio of 57·192′ to 61·165′, or of 1000 to 1069, should equal that of 1000 to 1034,—which it does not. It follows, therefore, that the earth's motion in its orbit is not uniform. But what law governs it? what relation exists between the angle it describes in a given time, and the distance at which it describes it?

Between the ratios we have just been stating there exists this remarkable relation. The distances are inversely as 1000 to 1034; and the angles as 1000 to 1069. Now if this last ratio is squared, it will become that of 1000000 : 1069156; and dividing both terms of it by 1000, it becomes 1000 : 1069, omitting the small fraction $\frac{156}{1000}$ in the last term. Now this ratio 1000 : 1069 is precisely that of the angles. The ratio of the angles is therefore equal to that of the squares of the distances taken inversely. Or, in other words, the angles described in the same time at aphelion and perihelion are inversely as the squares of the distances at which they are described. Whence it follows that if *the angle* described in aphelion be multiplied by the square of its distance, the product shall equal the angle described in perihelion multiplied by the square of its distance.

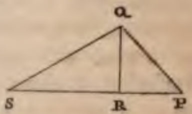
Now this law does not only obtain with regard to the motion of the earth in aphelion and perihelion, but in every other position of its orbit. If the angle described in a day, for instance, in any part of its orbit, be multiplied by the square of its distance, the product shall equal the angle described in a day in any other part of its orbit, multiplied by the square of its distance on that day.

In the following table will be found the angles observed to be described by the earth on the first days of the successive months of the year; and annexed to each is its distance on that day from the sun, in terms of the mean distance, which is taken as 10,000.

	Angle.	Dist.		Angle.	Dist.
Jan.	61′ 10″	9,830	July	57′ 13″	10,168
Feb.	60 51	9,860	Aug.	57 28	10,144
Mar.	60 5	9,920	Sept.	58 10	10,082
April	59 3	10,066	Oct.	59 7	10,001
May	58 6	10,088	Nov.	60 10	9,910
June	57 26	10,146	Dec.	60 56	9,860

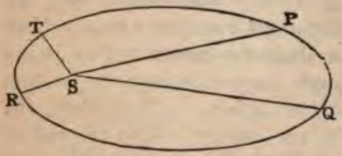
Now if the square of each of these distances be multiplied by the corresponding angle, the product will be found to be throughout the same. Generally, therefore, wherever the earth may be situated in its orbit, the angle it describes in a given time, a day for instance, being multiplied by the square of its distance, will always be the same quantity, viz. 59·128.

This is a very remarkable law. It was discovered by Kepler, and is the observed fact on which the whole of Newton's physical theory of the universe is made to rest. Kepler did not, however, leave his law in this form in which it first occurred to him. Let us suppose P and Q to represent positions of the earth at the interval of a very small portion of time; an hour for instance, or a minute. Also let S be the sun. The line PQ being exceeding small when compared with SP and SQ, may be considered a straight line, and SPQ a rectilineal triangle. Draw the perpendicular QR. This may be considered to be a portion of a circle described from the centre S, at the distance SQ or SP. QR will therefore equal the product of SP by the angle PSQ (SP × PSQ). Now the area of the triangle PSQ is equal to one-half the product of SP by QR ($\frac{1}{2}$ SP × QR); therefore the area of this triangle is equal to half the product of SP by the product of SP and PSQ, or to $\frac{1}{2}\text{SP}^2 \times \text{PSQ}$. Thus, then, the small triangular area PSQ swept over by the line SP, called the radius vector, in one minute of time, is equal to half the product of the angle PSQ by the square of the distance SP. And if this product be the same in every portion of the earth's orbit, it follows that the area swept over by the radius vector of the earth in every minute is the same, and therefore the area swept over in 60 minutes in one part of the orbit, is the same as that swept over in 60 minutes of any other portion of the orbit, and thus that the space or area swept over by the radius vector in one hour, in one day, or one week or month, in any one portion of the earth's orbit, is the same as the area swept over in any other. Now we have shown that the product of the angle described in a day by the square of the distance *is the same everywhere*. And precisely the same observations will *prove the same fact* in reference to the angle described in a minute. It

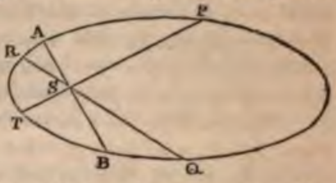


follows then, generally, that the area thus described by the earth in a given time in one portion of its orbit, is precisely the same as that described in the same time in any other portion. This is called the law of the equal description of areas. And it was in this form that it was promulgated by Kepler. We shall show, hereafter, that it results from this fact that the deflection of the earth from the rectilinear path which it would otherwise have in space, is produced by a force acting always towards the sun, and not at different periods towards different points in space, or at the same time towards different centres. It points out, therefore, with certainty the sun as the controlling power in the earth's motion, distinguishing it in this respect from all the other material existences which people space, but establishing nothing as to the law by which its influence upon it is governed.

The earth is in perihelion, or at its nearest distance to us, in December; it would seem, therefore, that at this season our weather should be hotter than at any other. It is at its greatest distance in July; about that time we should therefore expect the coldest weather. We know the contrary of this to be the case. How is this to be accounted for? The variations of the seasons have been explained as dependant, not upon actual variations in the distance of the source of light and heat, but upon the relative obliquities of the directions in which the rays of light and heat are received, and on the relative lengths of the periods during which they are received; but unquestionably these causes, although in themselves they sufficiently explain our alternations of heat and cold, admit of being modified in their results by other causes, and especially by a variation in the distance of the sun. We might, for instance, be brought so much nearer to the sun in winter than in summer, as to make the temperature constant. And unquestionably, our actual variation of distance is such as would produce this effect in *some* degree, were it not for another cause tending in a great degree to modify this. The earth moves faster round the sun, or with a greater angular velocity, when it is nearer to the sun, than when it is more distant. So that in traversing a given distance off in space it has not so much time to receive heat when near the sun as when more distant. This will be evident from a mere inspection of the diagram. Let PQ and TR be portions of the earth's orbit, described by it in the same time, say one month, the one near its aphelion, and the other near its perihelion; then, by Kepler's law of the equal description of areas, the areas RST and PSQ are equal to one another; and the two distances SP and SQ being greater than ST and SR, it is evident that the angle PSQ must be less than the angle RST, in order to make up this equality. Now the law by which the light and heat of the sun is communicated to the same body when situated at different distances is this; if at any distance you multiply the quantity (anyhow *measured*) of light and heat which it receives in a given time by the square *of its distance*, the product will be the same as though you multiplied the *quantity* of light and heat which it receives in the same given time at any



other distance, by the square of that distance. This is commonly expressed by saying that quantities of light and heat are inversely as the squares of the distances. But we have shown that if we multiply the angle described in any given time by the square of the distance, that product will equal a similar product taken in any other part of the orbit. The quantity of heat received in any given time varies, then, according to precisely the same law that the angle described in that time varies,—the relation of both to the distance is the same. And thus the quantity of heat received by the earth in describing the same angle is always the same. Thus, if PS and QS be produced to T and R, since the vertical angles at S are equal, there is as much heat received by the earth in moving from P to Q, as in moving from T to R. Or drawing the straight line ASB, the earth receives precisely as much heat from the sun whilst describing the space APB during the summer months, as whilst describing the portion BRA, which is its path in winter. Thus, that modification of the seasons which would otherwise be produced by our different distances from the sun is altogether got rid of, and the causes we have assigned for these phenomena exercise their full influence.

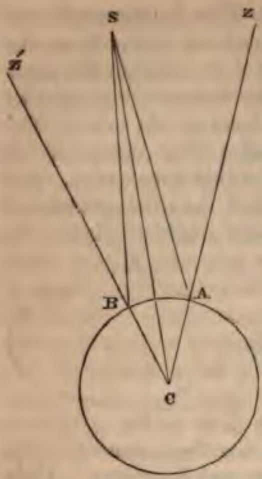


We have now described the form of the earth's orbit. Its position, too (which, not being a circle, but an oblong, is of importance), is easily known thus:—The earth comes to its perihelion on the 31st of December, as is known from the fact of its diameter being then the greatest, also the sun appears always in the opposite quarter of the heavens to that occupied by the earth. Ascertaining, then, what is the opposite place of the sun in the ecliptic on the 31st of December, and measuring from this 180 degrees, we know the precise position of the earth at that time, and, therefore, of the perihelion. The opposite quarter of the heavens is the aphelion; and thus we know which way in space the length of the elliptic orbit lies, and of course which way its breadth lies.

No. VIII.

The Dimensions of the Earth's Orbit.

Knowing the form and position of the earth's orbit, it remains now only to fix its actual dimensions. The general method by which the distance of the sun from the earth is found the reader will readily understand. Let A and B be two places on the earth's surface, which are on the same meridian of longitude. And suppose that at the same instant two observers ascertain the angular distance of the sun from the zeniths Z and Z′ of these two places. These angular distances will be the angles ZAS and Z′BS, which will, therefore, be known. Also the latitudes of the places of observation being known, the angle ACB, which is the sum, or difference of these latitudes, is known; and the dimensions of the earth being known, the radii CA and CB are known. Now having these quantities given, we can determine, by the rules of trigonometry, all the others which concern the quadrilateral figure ACBS; as will be *evident to any one* acquainted with that science. It may be, however,



made plain to those who are not, as follows:—If two lines, CA and CB, be taken, inclined to one another at an angle, ACB, equal to the sum or difference of the latitudes, and as many equal parts be measured off from CA as there are miles in the corresponding radius of the earth, and as many from CB as in the radius corresponding to B; also, if at the points B and A, thus determined, lines, AS and BS, be drawn inclined to AC and AB at angles equal to the observed zenith distances, ZAS and Z′BS, then the lines AS and BS will meet in some point, S, determined by the conditions under which the figure has been constructed; and this point S will hold the same position in respect to A and B and C that the sun does in respect to the two places of observation and the earth's centre. If, then, we find how many of the equal parts used there are in SC, we shall know the number of miles which the earth's centre is distant from the sun.

The method which has been described is one of the least artificial that can be conceived. In actual practice the compass and rule would fail us for want of accuracy, and even calculations thus made would not give even an approximation to the truth, by reason of the great length of SA and SB as compared with CA and CB. We must, therefore, have recourse to trigonometry, by a very simple operation of which we shall be enabled to determine SC, knowing CA, CB, BCA, Z′BS, and ZAS. It is thus ascertained that the mean distance of the sun is 23984 radii of the earth, or that SC is 23984 times AC. This result is confirmed by other and independent observations and calculations of a more complicated, and of a more accurate nature. We may, therefore, assume as a result, which cannot possibly be in error beyond certain known, and those comparatively very narrow limits, that the sun is distant from us 95 millions of miles. The greatest diameter of the earth's orbit is equal to twice its mean distance, or 47,968 radii of the earth. Now it has been shown that the sun's distance from the two extremities of this orbit are in the ratio of 32,593 to 31,517; this being the ratio of its apparent diameters in aphelion and perihelion, we have only then to divide the number 47,968 into parts which are in the ratio of 32,593 to 31,517, and we obtain for the earth's distance in aphelion 24,388 radii of the earth; and for its distance in perihelion 23,580 radii. Knowing, thus, the position of one of the foci of the earth's orbit, and the length of its greater diameter, we can determine all its dimensions. Thus, then, the magnitude of the earth's orbit is completely known.

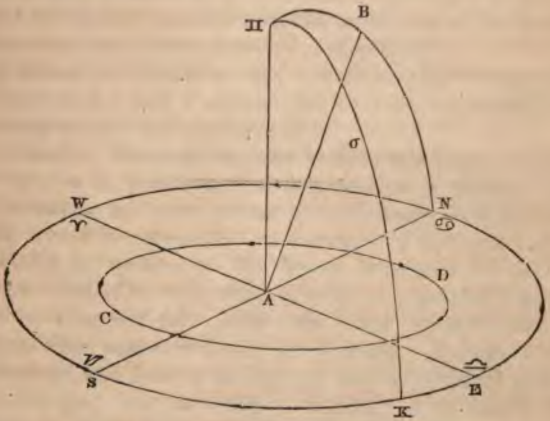
No. IX.

THE PLANE OF THE EARTH'S ORBIT.

In our reasonings hitherto we have supposed the observer to occupy *a position* on the earth's surface, and everything presented to him under ese circumstances has necessarily been presented under a complicated rm, combining with its proper motion another *apparent* motion, arising om a continual change in the observer's point of view; to separate these ro motions is a distinct operation of the mind, and one of considerable fort.

The mind may now, however, take up a new position in the iiverse, and in imagination move at will on the surface of a fixed variable plane, that of the earth's orbit, which when produced in the ult of the heavens, traces out there the ecliptic. Let a line be awn perpendicular to this plane through the sun, and it will mark the heavens a point called the pole of the ecliptic; great circles of e heavens drawn through this pole perpendicular to the ecliptic are lled circles of celestial latitude, and the latitude of any point in the avens is the number of degrees between that point and the ecliptic, easured on one of these circles.

If a line be drawn through the sun parallel to the direction the earth's axis, and through this line a plane perpendicular the plane of the earth's orbit, this plane will intersect,—the heavens in a rcle called the solstitial colure,—and the line of the earth's orbit in the ints which we have before shown to be those occupied by its centre at e solstices[*]—and the celestial ecliptic in two points called Cancer ♋ and apricorn ♑, in which points of the heavens are the sun's apparent places the time of the solstices. If from these points were measured off 90 grees both ways on the ecliptic, we shall determine two points in it lled the equinoctial points, ♎ and ♈, where the sun appears on the uinoxes. The number of degrees from Aries ♈ eastward of the point here the circle of latitude of any place in the heavens cuts the ecliptic, called the longitude of that place of the heavens.



Thus, if NESW be the intersection of the plane of the earth's orbit with the sphere of the heavens, it will be the celestial ecliptic, and if Π be perpendicular to this plane, Π will be its pole. If AB be parallel to the earth's axis, and the plane ΠBNS perpendicular to SKN, the intersection, S Π N, of this plane with the sphere of the heavens will be the solstitial colure, and S and N will be the solstices Capricorn ♑ and Cancer ♋, and the south and north points of the ecliptic; 90 degrees from these will be the points ♈ and ♎, of which the former is the western and the latter the eastern point of the ecliptic. The latitude of any point σ of the heavens is the arc σ K, and its longitude the arc ♈ N K, or the angle ♈ SK.

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[*] *These are the* northern and southern points of the earth's orbit.

The sphere of the heavens which we are now describing is in reality a different one from that before spoken of; that sphere had its imaginary centre in the centre of the earth; this has for its centre the centre of the sun; so that the centre of its former sphere was in motion, and its motion was perpetually *round* the centre of this sphere. But it was shown that the radius of the circle in which this motion takes place is infinitely small as compared with the radius of the great sphere of the heavens; so that, so far as the appearance of objects on its surface was concerned, it might be considered to be at rest. So far as the appearance of these objects are concerned, it may therefore be supposed to coincide with the centre of that sphere of which we are now speaking. Considering then the centres and surfaces of these spheres to coincide, we shall have two sets of lines on the celestial sphere, the one having reference to the equinoctial, and the other to the ecliptic, and two poles, one being that of the former, and the other of the latter. The right ascension and declination of any heavenly body or point in the heavens is measured and fixed by means of the former set of lines, precisely as its longitude and latitude are by means of the latter.

GEOLOGY AND THE HOLY SCRIPTURES.

[We have received from a reverend gentleman, highly distinguished both for his piety and his learning, the following communication on the subject of our review of Dr. Buckland's *Bridgewater Treatise*. As a scriptural and philological argument, it might, by strict construction, be deemed somewhat out of place in the *Magazine of Popular Science;* but a consideration of the importance of the subject, and the interest which it is peculiarly calculated to excite in the minds of religious readers, particularly when so able and influential a person as the writer of this paper has entered into the discussion in our own pages, have decided us to waive this distinction, and to present it to our readers, without remark or comment.]

To the Editor of the Magazine of Popular Science.

Sir,—With cordial approbation of the design and the general execution of your article, in the last month, upon *Dr. Buckland's Bridgewater Treatise*, I request your candid indulgence of some brief remarks.

The observations in pp. 337—339, appear to me capable of being misunderstood, or of being construed injuriously in various ways to the interests of both science and religion. The tendency of those observations appears to be, First, to assume (or at least to warrant the assumption) that the Holy Scriptures contain allegations and implications with respect to the natural history of our earth, which are contradicted and disproved by the demonstrations of modern geology; and, Secondly, that it is the duty of a philosopher to abstain from any discussion of this discrepancy, and from any inquiry whether it be real or only apparent: as if it were said, Let these two branches of knowledge be kept far away from each other: let philosophers and geologists pursue their own course, and let theology and religion practise their own duties, and watch over their own interests; but let neither interfere with the other; let no inquiry ever be made whether they are in accordance or in opposition.

This short way of dismissing the matter has, indeed, been adopted by some eminent men; but I appeal, Sir, to your impartial reflection, whether it is not *absurd* and *impracticable.*

1. It is *absurd.* Truth throughout her whole domain, illimitable as is its extent, is one in principle, and harmonious in details. It is no other than the having our conceptions in accordance with the reality of things. And *Truth in expression* (= veracity) is the adapting of our language, written or spoken, to the honest utterance of our conceptions. A mere child, if he will reflect a moment, perceives that a proposition cannot be true and false, under the same circumstances; unless there be some artifice practised in the use of terms. An assertion cannot be true in theology, and false in geology, or any department whatever of scientific knowledge; nor inversely. It really is an insult to men's understandings, to admit indirectly, that there are affirmations or doctrines in the records of revealed religion, which are disproved by the clearest evidence of science; and then to proscribe investigation, with a solemn pretence of mysteries not to be inquired into, an hypocritical tone of *reverence* for sacred things. The veil is transparent; no man *can be deceived by it:* but it is lamentable that any should attempt to

deceive by it. We greatly wrong the interests of knowledge, and prejudice our own improvement, when we but seem to admit that theology is an insulated portion of science, which may be safely pursued by itself, and which yields no advantages to other departments. True theology, on the contrary, attracts to itself, illustrates, and harmonizes all other knowledge. It is the science which relates to the Author and Preserver of the whole dependent universe; whatever may be known concerning HIM, for the noblest purposes of intellectual improvement, of personal virtue, and of diffusive happiness. It is formed by strict induction from the works and the word of God; natural notices, and positive revelation. It is the friend of all science; it appropriates all truth; it holds fellowship with no error.

2. It is *impracticable*. This kind of ban upon a reasonable, an inevitable query, is never submitted to by any person of sound understanding. Either he receives the assumption,—and, as its consequence, he rejects covertly or openly the truth and authority of the Bible; or he searches out the matter fairly and fully, and then he learns that the assumption is false.

Is it then the fact, that such fair and impartial inquiry will bring out this result? Is it, after all, an erroneous assumption, that the declarations of Scripture and the sensible demonstrations of geological science, pointedly contradict each other? Does not the Bible teach that the moment of the Supreme Being's first putting forth his creating power, was only about six thousand years ago? And do not the undeniable phenomena of stratification, and other facts, demonstrate that our globe (to say nothing of the rest of the solar system, and the astral universe,) has existed, has passed through countless changes, such as are continually in progress, and others of a more intense character, which rational estimation must suppose to have required a period for their production so vast as to fill us with astonishment,—which no calculator ventures to lay down,—which probably amounts to millions and millions of years?

Fully admitting the assumptions in the last query, I deny that of the preceding one.

It is to be lamented that the common habits of expression nourish the opinion, that the authority of Scripture maintains the commencement of dependent nature to have been as has been stated: and it is scarcely less to be lamented that theories have been propounded for conciliating the facts of nature and the Scripture narrative, which rest upon either a defective acquaintance with those facts, or a disregard to the plain use of language in that narrative. Of the former kind are the schemes for finding the time requisite for the terrene formations, in the period from the creation of the first man, to the Noachian deluge; of the latter, those which interpret the *days* of successive operation, laid down in the primeval record, as if they were indefinite periods.

It will appear evident to any one who will reflect upon the case, that the records of revelation must have been written in the phraseology and idioms of the people and the age to which they were given; or they would have been unintelligible. Upon this principle we account for the *manner* in which natural phenomena are currently described; and for the expressions which impute to the Infinite Spirit the form, the organs, and the mental affections of a human being; and various other characteristics of the parabolic style of the Hebrew Scriptures. Such language was a condescension to the infirmities of mortals, and best adapted to the instruction of the general mass of mankind: but it is self-evident that it must be interpreted in a manner congruous with the perfect attributes of the Deity, and the reality of things.

A philological survey of the initial section of the Bible (Gen. i. 1, to ii. 3.) brings out the result:—

i. That the first sentence is a simple, independent, all-comprehending, axiom, to this effect—that *matter*, elementary or combined, aggregated only or organized, and *dependent sentient and intellectual beings*, have not existed from eternity, either in self-continuity or succession, but had a *beginning;* that their beginning took place by the all-powerful will of ONE BEING, the Self-existent, Independent, and Infinite in all perfections; and that the date of that beginning is not made known.

ii. That, at a recent epoch, our planet was brought into a state of disorganization, detritus, or ruin, (perhaps we have no perfectly appropriate term,) from a former condition.

iii. That it pleased the Almighty, Wise, and Benevolent Supreme, out of that state of ruin, to adjust the surface of the earth to its now existing condition; partly by the operation of the mechanical and chemical causes (what we usually call *Laws of Nature*,) which Himself had established; and partly, that is, whenever it was necessary, by His own creative power, or other immediate intervention; the whole extending through the period of six natural days.

It has been indeed maintained, that the conjunction *and*, with which the next sentence begins, connects the succeeding matter with the preceding, so as to forbid the intercalating of any considerable space of time. To this we reply, that the Hebrew conjunction, agreeably to the simplicity of ancient languages, expresses an annexation of subject or a continuation of speech, in any mode whatever, remote as well as proximate. For denoting such different modes of annexation, the Greek and other languages have a variety of particles; but their use is in Hebrew compensated by the shades of meaning which the tone in oral speech, and the connexion in writing, could supply. To go no further than the first two leaves of the Hebrew bible, we find this copula rendered in our authorized version, by *thus*, *but*, *now*, and *also*.

This interpretation is what I have been labouring to diffuse for more than thirty years, in private and in public, by preaching, by academical lecturing, and by printing. But it is not my interpretation, though I believe that I originally derived it from the sole study of the Bible-text. Clemens of Alexandria, Origen, Basil, Chrysostome, and Augustine, among the fathers (though not in a truly philosophical way, which was not to be expected), departed from the vulgar notion: and some judicious interpreters of the sixteenth and seventeenth centuries have done the same, in particular, Bishop Patrick and Dr. David Jennings. Of modern Scripture critics I say nothing; for prejudice, justly or unjustly, may lie against them. Not that the question is to be settled by human authority. Our only appeal for decision is to the *Bible itself, fairly interpreted*. But the mention of venerable names may be useful, to allay the apprehensions of some good persons, who only hear obscurely of these subjects, and have not the means of forming an independent judgment on solid grounds.

I, therefore, with many, feel greatly obliged to Dr. Buckland for having come in aid of this, which I believe to be, *the true sense and meaning* of the sacred writers. I am framing no hypotheses in geology; I only plead that *the ground is clear*, and that the dictates of Scripture *interpose no bar* to observations and reasonings upon the mineralogical constitution of the earth, and the remains of organized creatures which its strata disclose. If those investigations should lead us to attribute to the earth, and to the other planetary and astral spheres, an antiquity which millions or ten thousand millions of years might fail to represent, *the divine records forbid not their deduction.* Let but the geologist maintain what his science so loudly proclaims, that the universe around us has been formed, at whatever epoch, or through whatever succession of epochs, to us unknown, by the power and wisdom of an Almighty first cause. Let him but reject the absurdities of pre-existent matter, of an eternal succession of finite beings, of formations without a former, laws without a lawgiver, and nature without a God. Let him but admit that man is but of yesterday, and that the design of revelation is to train him to the noblest purity and happiness in the immortal enjoyment of his Creator's beneficence; and he will find the doctrines of the Bible not an impediment, but his aid and his joy.

I have written much more than I anticipated, and I will tax your indulgence no longer; otherwise, confirmation and illustration might be brought from various passages of Scripture, and it would plainly appear that a just interpretation of the idioms of the Hebrew language, marked with archaic simplicity, would show them to be susceptible of an unforced accommodation to philosophical truth; just as, in every modern language, phrases of current parlance, which, literally taken, would be absurd, are continually used by the masters of science as well as by common men. In such cases, error is neither given nor taken, and to affect philosophical precision would be miserable pedantry. This general principle may, I humbly think, be satisfactorily applied to the account of the Noachian Deluge, and to the obviating of some of its difficulties, though others will probably remain as a proper test of our disposition to rely implicitly on the infinite wisdom, goodness, and power of the glorious Author and Preserver of all things; "in whose hand are the deep places of the earth, and the strength of the hills is His also."

J. P. S.

Dec. 10, 1836.

QUESTIONS FOR SOLUTION RELATING TO METEOROLOGY, HYDROGRAPHY, AND THE ART OF NAVIGATION.

By M. Arago.

[Continued from p. 397, vol. II.]

PHENOMENA OF THE SEA.

On the means of drawing up Sea-water from great depths, and of ascertaining in what proportion the two principal constituents of Atmospheric Air are contained in it*.

Chemists have long since proved, that water becomes impregnated with the gases which rest on its surface. This absorption takes place in consequence of true chemical affinities existing between the water and the different gases; when their effects on oxygen and azote, the two principal constituents of atmospheric air, are carefully examined, the affinity is found to be much stronger with regard to the first than the second. Hence it follows, that the waters of seas and rivers, being always in contact with the atmosphere, become at length impregnated with gaseous mixtures, in which oxygen predominates. Indeed, the very accurate experiments of MM. von Humboldt and Gay-Lussac have proved that rain-water, the water of the Seine, and snow-water, contain a mixture of oxygen and azote, in every 100 parts of which there are from 29 to 32 of oxygen; though the proportion of oxygen in atmospheric air is constantly equal to 21 parts only, and that in all seasons and climates. MM. von Humboldt and Provençal have in addition to this ascertained, that the absolute volume of mingled gases contained in water near the surface is $\frac{1}{36}$ of the volume of water.

It follows as a necessary consequence of these properties, that the vast extent of sea which covers a large part of the globe, is impregnated with a mixture of gases, the proportions of which, near the surface, must be similar to those just mentioned. I have ascertained that it is so at the depth of eleven hundred yards; for, in an experiment I formerly made in the Mediterranean, sea-water, drawn from that depth, yielded a mixture which contained 28 parts of oxygen in every 100.

But here several important questions in terrestrial physics present themselves, which cannot be solved by the apparatus I then employed. In proportion to the descent into the sea, does the pressure of the superior portion upon the inferior become greater; and as a column of sea-water eleven yards in height, is nearly of the same weight as a column of air of an equal base extending from the surface of the earth to the limit of the atmosphere, it follows that at a depth of eleven hundred yards the water sustains a pressure of a hundred atmospheres. How enormous, then, must this pressure be on beds still lower, if the mean depth of the sea, at a distance from the coasts, extends to several miles, as the laws of gravitation seem to indicate†! It has also been proved, by direct experiment, that water, whose surface is in contact with compressed gases, and which sustains their pressure, absorbs the same volume of these gases as if they were subjected to the simple pressure of a single atmosphere; so that the weight absorbed becomes proportionably greater. If, then, the single fact of a uniform absorption, propagated from one bed to another throughout the whole mass of waters, be sufficient to account for the presence of a considerable volume of air, how greatly may the quantity be increased if it should be in proportion to the pressure due to each depth! As this saturation must have been in gradual operation ever since the seas were formed, it must also have modified gradually the pre-existing atmosphere, and perhaps continues to affect the present one if the affinity which produces the saturation is not satisfied. The influence of these phenomena on the state of the atmosphere, and, consequently, on the conditions of existence of the living beings on the surface of the globe, is amply sufficient to induce us to examine them, and to measure the extent of their operation.

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* This part of the subject was drawn up by M. Biot.

† *Mécanique Céleste*, tom. ii. p. 200.

For this purpose, it is desirable to obtain sea-water from great depths, far from land, and to bring it to the surface with all the air which it contains. This air must then be disengaged by boiling, its volume measured under the ordinary pressure of the atmosphere, and finally subjected to chemical analysis. In these operations, the only difficult one is that of drawing the water from the desired depth, and bringing it to the surface with all its contents. First, care must be taken not to employ vessels which are exhausted, or filled with air only, designed to open and admit the water at the assigned depth; for the pressure to which they must be subjected before they are deep enough, will cause the water to filter through the joints of the most perfect plugs, or crush the vessel if these resist. And, secondly, if the gaseous mixture contained in the deep-lying beds is subjected to the same pressure which they undergo, it will expand inversely when the apparatus is brought near the surface, and will either escape by the plug, or burst the vessel containing it. In order to avoid these contrary effects, a hollow glass cylinder ought to be employed, closed at one extremity by a solid plate of metal, thus forming a complete bucket provided with a handle, to which a cord is attached to let it down to the depths of the sea. This bucket being empty, and open to the surrounding water, descends into the different beds without being injured by their pressure. When it has reached the required depth, another cord is pulled, this is attached by an inverted handle to its lower part, and serves to invert it. This second cord is then employed to draw up the apparatus, and in order that it may not get entangled with the first, it is worked from the other end of the ship. The cylinder of glass has two bottoms, one fixed, the other moveable. The latter is in reality the piston of an air-pump, which descends by its own weight, when the bucket is drawn up; at the same time, the fixed bottom has a small hole, furnished with a valve, opening inwards by the pressure of the surrounding water, and allowing it to enter into the empty space made by the descending piston. When this has completed its descent, and the space is filled, the valve in the fixed bottom closes by its own spring, and the admitted water is thus separated from all other during the drawing up. But if this water contain compressed air, nothing can counteract its expansive tendency, or that of the air it contains.

when it is brought to the surface, where the pressure of water externally is removed; it will then either escape or burst the apparatus. To guard against this, a free issue must be provided for all possible expansion either of the water or air. For this purpose the fixed bottom is furnished with a lateral tube which leads to a gas-bladder: the latter having been first filled with water, then emptied and pressed together before the sinking of the apparatus. This bladder will receive all the air which may be disengaged from the water on approaching the surface; and, if any be so disengaged, will return more or less inflated. Then by closing the stop-cocks with which the tube is provided, the bladder may be separated from the vessel containing the water, its volume measured, and the enclosed air analyzed; after this, the air which may still remain in the water may be examined, and also all the substances which the water may hold in solution. Such is the apparatus which has been intrusted to the commander of the *Bonite;* and the zeal, as well as the intelligence, of that officer, affords us the assurance, that, under his directions, it will be usefully employed to solve the various questions indicated above, relating to terrestrial physics; questions which, besides their purely scientific interest, have an additional importance attached to them, by the knowledge which their solution would supply respecting the permanence or variability of our atmosphere, and the conditions of existence of the animated beings which exist in the depths of the sea.

MARINE CURRENTS.

The Atlantic and Pacific Oceans, and the Mediterranean Sea, are traversed by numerous currents, which are the more dangerous, as they carry vessels out of their proper course, without the pilot suspecting it; in cloudy weather, particularly, he has no means of ascertaining their influence. Among the phenomena of the sea, and considered in their twofold connexion with theory and practice, there are certainly none more deserving than these of a high degree of attention by navigators of every country. The numerous memoirs and works specially appropriated to the subject, such as those of Ducoudray, Romme, and even the posthumous and scientific treatise of Major Rennell, which has recently appeared, are very far, in my opinion, from having exhausted the subject. Of this the reader will, finally, be able to judge.

ON THE CAUSE OF CURRENTS.—The most remarkable currents observed by navigators are, in the Atlantic:—

The current, which, having gone round the Bank of the Agulhas and the Cape of Good Hope, proceeds from south to north along the western side of Africa, as far as the Gulf of Guinea.

The current, termed equinoctial, which runs invariably from east to west on both sides of the equator, between Africa and America.

The current which, after having issued from the Gulf of Mexico by the Straits of Bahama, runs at a certain distance from the coasts of the United States in the direction of N.E. as far as the Bank of Nantucket, where its direction is changed.

Lastly, the current, by the action of which the waters of the *Ocean which* wash the coasts of Spain, Portugal, and Africa, from Cape Finisterre as far as the parallel of the Canaries, are all directed towards the Straits of Gibraltar.

What is the cause of these currents?

The trade-winds, according to some, by continually blowing from east to west in the Indian Ocean, must produce a *liquid intumescence* on the eastern coast of Africa near the equator. This accumulated water flows continually from north to south through the Straits of Mosambique. When it reaches the parallel of the Cape of Good Hope, the eastern wall or mound which had hitherto maintained it being discontinued, the water necessarily flows westward. It is thus that it forms the current of the Agulhas.

The equinoctial current of the Atlantic is attributed to the constant impulsion of the trade-wind on the waters in the vicinity, and to the north and south, of the equator.

The Atlantic equinoctial current, in this respect resembling the equatorial current of the Indian Ocean, must produce a great accumulation of water along the first coast which presents itself as a barrier; this coast is America. Hence results a general movement of the Caribbean sea towards the strait which separates the eastern point of Yucatan from the western point of Cuba; this produces an elevation of the level of the sea in the Gulf of Mexico; and this elevation, finally, is the cause of the rapid formed by the accumulated water in the Gulf, at its escape from the Strait of Bahama, the prolongation of which becomes the *Gulf-stream.*

With respect to the current in the Straits of Gibraltar, it might be caused by a depression of the level of the Mediterranean, and this depression might be occasioned by an excessive evaporation, which the influx of the tributary rivers is insufficient to compensate.

These explanations are simple; they appear to rest on physical causes the action of which must take place in the direction that is assumed; and the most intelligent observers, Franklin, Rennell, &c., have adopted them; yet, I am about to prove that they are not so completely established by observation, measurement, and experiment, as to prevent us from entertaining legitimate doubts on the subject.

A continued and strong wind raises the level of the sea along the coast towards which it tends to direct the water; thus, at Brest, Lorient, Rochefort, &c., the tide is always highest, all other circumstances being equal, during a *west wind.* So on the opposite shores of the Atlantic, and along the coasts of the United States, it is, on the contrary, the *east* wind which produces this effect. So it is by *south* winds that the waters of the Mediterranean are accumulated in the ports of Genoa, Toulon, and Marseilles, and by *north* winds in those of Algiers, Bugia, and Tunis. These facts are not disputed, nor do they admit of being called in question. It only remains to determine the value of the accidental changes of level which winds may produce.

Franklin relates that, in an extensive piece of water three leagues broad, and about three English feet in depth, a strong wind caused the whole of one of the sides to become dry, while it raised the water on the opposite side three feet above its former level, the depth there being six feet instead of three. In our own seas, I do not think that in general, a higher number than this should be stated as the maximum effect produced by the most violent tempests*.

The Trade-winds are certainly constant, but their strength is extremely moderate. The depressions of the sea-surface which they occasion must, therefore, be inconsiderable. It seems difficult to admit, that the vertical fall of a yard, for example, or even two yards, can produce currents which are not entirely annihilated after a passage of many hundreds of leagues.

I have stated that the trade-winds, on account of their feeble intensity, seem little likely to produce any considerable intumescence in the waters of the ocean. I shall even go further, and prove, that, in point of fact, the very seas from which currents appear to emanate, are exactly, or very nearly, of the same level as those which these currents afterwards traverse.

It has been indisputably proved by M. Lepère, by observations made during the Egyptian expedition, that the level of the Mediterranean near Alexandria, is lower by 26·5 feet than the low water level of the Red Sea near Suez, and by 32·5 feet than the high water level at the same place.

This is certainly a very great difference of level between two seas which may be considered as communicating with each other; for, on the one hand, the Mediterranean opens into the Atlantic by the Straits of Gibraltar; on the other, the Red Sea joins the Indian Ocean by the Straits of Bab-el-mandel; and, thirdly, the Atlantic and Indian Ocean blend with each other at the Cape of Good Hope. It is very far from my intention to depreciate what is curious or interesting in such a result as this; but I must be allowed to say, that it throws no light on the disputed question of currents, for, to render the explanation admissible, there ought to be a sensible difference between the level of two contiguous seas, between that from which the current issues, and that into which it flows.

Further, has a difference of level been clearly proved to exist between the Gulf of Mexico in which the Gulf-stream originates, and that part of the Atlantic Ocean which washes the eastern side of the Floridas and Georgia?

The inhabitants of the Isthmus of Panama believe, but without proof, that the Pacific is higher than the Atlantic Ocean. Franklin, Rennell, &c. likewise admit a difference of elevation, but of an opposite nature. M. von Humboldt confirmed this latter opinion by barometrical observations made at Cumana, Carthagena, and Vera Cruz, compared with others made at Acapulco and Callao. At the three places first mentioned, the waters appeared to him to be about ten feet *above* the level of the Pacific, as taken on the western shores of Mexico and Peru. Now, as no one doubts that the general mass of the Pacific and Atlantic Oceans has the same level, that portion of the latter near the Antilles, and that which is enclosed in the Gulf of Mexico will thus form a local elevation or intumescence of about ten feet.

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* Places are mentioned in the Mediterranean, where gusts of wind from the *south-west* (*called Labeschades*) have raised the waters twenty-three feet above their ordinary level; but these effects are entirely local.

Before citing a work which does not confirm this result, I ought to mention that my illustrious friend has himself remarked, with his usual caution, that his observations were not sufficiently numerous to place the fact of so small a difference of level beyond doubt.

Two engineers have lately crossed America at its narrowest part, in order to settle definitively the relative elevation of the two oceans. We may add that their object was not purely of a scientific nature, but had a direct reference to one of the grandest problems which commerce ever proposed,—the possibility of a communication between the Atlantic and the Pacific, across the Isthmus of Panama. Such was the object of the investigation, the results of which I am about to state, and which was intrusted by General Bolivar to Mr. Lloyd, an English engineer, and to a Swedish captain, named Falmarc.

This survey was made in 1828 and 1829. The level used was constructed by Carey, of London. The line commenced at Panama, on the Pacific Ocean, at the level of the highest tides of the equinox, corresponding to the third day of the full or of the new moon. Its termination was at a place named Bruja, to which the influence of the tide extends. Bruja is on the river Chagres, about twelve miles from the place where that river enters the sea of the Antilles.

At Panama, the mean difference of the levels of high and low water, during spring tides, is 21·2 feet. At Chagres, on the Atlantic, this difference does not exceed 1·1 foot.

By assuming, in each place, for the mean level of the ocean, a surface equally remote from the successive levels of high and low water, it follows, from the survey of Messrs Lloyd and Falmarc:

1*st*. That the mean level of the Pacific Ocean, at Panama, is 3·52 feet *higher* than the mean level of the Atlantic Ocean at Chagres.

2*nd*. That at the instant of high water, the Ocean on the western coast of the Isthmus, is 13·55 feet *higher* than on the eastern coast.

3*rd*. That, on the contrary, at the instant of low water on the same coasts, the Pacific Ocean is *lower* than the Atlantic.

These observations, seem, then, to confirm the opinion long since adopted, that the mean level of the Pacific is more elevated than the mean level of the Atlantic; but the difference, instead of being enormous, as was supposed, is only 3·52 feet. It may even be permitted to suppose, without injustice to the merits of Messrs. Lloyd and Falmarc, that, in carrying their operations through a wild country abounding with difficulties, in running a line, whose total extent, including sinuosities, is eighty-two miles, and that, in observing at 935 stations, they may have erred to the small extent of a yard and a half. It would then follow, that there is nothing to prove that there is any sensible difference between the mean levels of the two great seas which communicate with each other by the Straits of Magellan and Cape Horn*.

The labours of Messrs Lloyd and Falmarc, so far at least as they apply to the explanation of the rapid current which precipitates itself from the Gulf of Mexico into the Ocean by the Straits of Bahama, would prove, hypothetically, that the Pacific and Atlantic Oceans, viewed as a whole, form a surface of the same level. We shall escape from this difficulty by relating the results of some observations made in Florida a few years since, by the French officers appointed by the American Congress, to survey the line of a canal designed to unite the river St. Marie, on the Atlantic, with the bay of Appalachicola, on the Gulf of Mexico.

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* If, after the scientific memoirs of M. *von Humboldt*, it is still necessary to *return to the* truly astonishing depression *that the Cordilleras* of South America *present in the* Isthmus of Panama, before they again assume their full majesty in Mexico, I would remark, that the most elevated point of the transverse line surveyed by Messrs. Lloyd and Falmarc, is only 633 feet above the level of the sea.

According to the first result of the measurements, *low water* in the Gulf of Mexico would be higher than the *low water* of the Atlantic, by 3·73 feet. A second result gave a difference of the same nature between the two low waters, of 2·8 feet. The mean is 3·28 feet.

But even this slight inequality of level is greater than the real one. In fact, when we compare two seas subject to tides, it is evidently the mean levels, that is to say, the surfaces, equally remote from high and low water, that ought to be compared. In this instance, although I can perceive no reason for it, the comparison was made between two low waters. In order to state the matter accurately, therefore, it is necessary to elevate the surface taken for comparison in the Gulf of Mexico to half the height of the tide observed in that gulf. The same thing must be done in regard to the eastern or Atlantic side of the Floridas. In the gulf, near the point where the level was terminated, the tide does not rise more than about ten inches. On the other side of the Floridas, near the mouth of the river St. Marie, the tide is about 6·56 feet. Low water, therefore, is 2·63 feet more removed from the mean tide at St. Marie than in the gulf. If, then, the mean levels are referred to, as must be done to obtain the real result, instead of 3·28 feet, it will be found that the difference of the level of the two seas is 3·28 *minus* 2·63 feet, that is to say, 0·65 feet, or about eight inches.

This quantity is evidently within the limits of error, which observations embracing the whole breadth of the Floridas must be liable to. But even though the difference alleged were real, it may be doubted whether any one would now be inclined to regard an inequality so unimportant as a sufficient explanation of the cause of a current which, issuing from the Straits of Bahama, at a rate of not less than five miles an hour, continues its progress nearly in a straight line into the very middle of the Atlantic, to a distance of about 1200 miles, without its rapidity abating during so long a course.

Let us now consider the Mediterranean. In this sea the alleged lowness of the level,—the presumed cause of the current flowing from the Ocean towards the Straits of Gibraltar, is said to be the result of an enormous annual evaporation, which the mass of waters contributed by the Nile, the Rhone, the Po, &c. is insufficient to compensate. Direct and demonstrative proofs of this want of compensation are, it is true, completely wanting. But if this objection be advanced, a new form is immediately given to the argument, and then it is said (which is in reality the case), that in summer, at equal latitudes, the waters of the Mediterranean are about 5½° to 6½° Fahr. warmer than those of the Ocean, from which *it inevitably follows that* the first undergo more evaporation than the *others, and that nothing* more is required to explain the current of the *Strait.*

And this, it must be confessed, would be sufficient, if the cause indicated were to produce a *very sensible* difference of level in the two seas. Thus, whatever may have been said of it, the problem will be found to be reduced to one of numbers, or to a question of fact. It must be ascertained, either by calculation or experiment, *to what extent* the Atlantic Ocean is higher than the Mediterranean. The calculation, I have already stated, will be difficult to be made with precision, owing to the want of sufficient data. With regard to experiment, the results of that which I am about to present, seem to me calculated to satisfy the most scrupulous minds.

Delambre had already found, by the great chain of triangles on the meridian which extends through France, from Dunkirk to Barcelona, the means of directly connecting the level of the two seas. The triangles comprehended between Rhodez and the Mediterranean, gave him for the vertical height of that town, a result which agreed to a fraction of a metre with the height, referred to the Ocean, that was obtained from that portion of the chain placed between Rhodez and Dunkirk.

It has already been stated in opposition to this result, that the observations from which it was deduced, were not always made under favourable circumstances; that they should have been many times repeated, if intended to be decisive of a difference of level; and that, moreover, the necessary calculations had neither been made with care, nor by methods sufficiently accurate. These objections were not without weight: and for this reason the officers of the corps of *Ingénieurs-géographes* endeavoured to take advantage of the chains of the primordial triangles which stretch in different directions and cover the whole surface of France, in order to submit the question of the levels of the two seas to a new examination. M. Delcros, among others, devoted himself to the subject. He made extensive investigations, but which, however, are still in MS., and I have therefore to regret that I am unable to state the results in this paper. But the observations which M. Corabœuf has presented to the Academy of Sciences, are also as directly to the point as could be desired, and were conducted with a precision which it would appear difficult to exceed.

This operation, carried on along the southern frontier of France, during the years 1825, 1826, and 1827, embraced, in the line of the shortest distance, the whole of the interval lying between the Ocean and the Mediterranean. Forty-five primordial triangles, many of which have their vertices on the highest peaks of the Pyrenees, join the fort of Socoa, near St. Jean de Luz, to several points of the plain of Perpignan, the small elevation of which above the sea was obtained by two secondary triangles. All the angles were measured by M. Gambey's repeating-circle, and were repeated three times at the least. The same was the case with the zenith distances. Care was taken, also, to make the observations between 10 A.M., and 3 or 4 P.M., only, in order to avoid the effects of irregular refraction, which takes place near the horizon some hours after sun-rising, and before his setting. The amount of atmospheric refraction, between each couple of stations, was deduced from a comparison of reciprocal zenith distances. As assistants in these important operations, M. Corabœuf had Captain Peytier and Lieutenants Hossard and Testu of the corps of Ingénieurs-géographes.

The station at *Crabère* is nearly in the middle of the interval which separates the Ocean from the Mediterranean. The eastern part of the chain of triangles served to determine its height above the Mediterranean; the other part gave the height above the Ocean. It is important to remark, that the calculations could have been made by a variety of distinct combinations, among which M. Coraboeuf made choice of three. He ascended, in the first place, from the Ocean and the Mediterranean to Crabère, following the single series of vertices which bounded the chain on the south; then, secondly, by taking exclusively the northern vertices; then, thirdly and fourthly, by travelling diagonally, that is to say, by visiting alternately a northern vertex and a southern one. The following is the result of these various combinations:—

	Height of Crabère.		
	Above the Mediterranean	Above the Ocean.	Diff.
By the line of the southern vertices . .	2633^{m}, 37	2632^{m}, 95	0^{m}, 42
By the line of the northern vertices . .	2633 , 99	2632 , 07	1 , 92
By the first diagonal line	2633 , 87	2633 , 61	0 , 26
By the second diagonal line	2632 , 79	2632 , 49	0 , 30
Means	2633 , 50	2632 , 77	0 , 73

The mean difference, 0^{m},73 (2·4 Eng. ft.), is so small, particularly when we recollect the extent of the line which was levelled, that it cannot prevent the conclusion that, in a state of repose, the waters of the Ocean, and those of the Mediterranean, have a surface of the same level. At all events, there can be scarcely a doubt, that if any difference in this respect does exist, it is too small to be appreciated.

In this article I wish merely to prove that the subject of currents is far from being exhausted; that differences of level, to which hydrographers have recourse for an explanation of them, are either completely nugatory, or insignificant; and that there is still room for ample investigation. This object I conceive I have attained. I shall still, however, add a few reflections.

The theory of currents has made little progress hitherto, because those phenomena have chiefly been considered which affect the surface of the sea. Currents produced by differences of saltness and of temperature exist at all depths. There are currents, for example, in contact with the very bed of the sea, which transport the cold waters of the polar zones as far as the equator. Near the poles these waters, like the solid part of the earth that supports them, move at a very slow rate, from west to east. As they pass by degrees to temperate and warm regions, they arrive at greater terrestrial parallels, which thenceforth move quicker than they, and hence the relative currents which run from the east to the west, and of which the volume is equal to that of the polar currents.

It is, if I am not deceived, by placing them in this point of view; by descending, in imagination, to the profoundest depths of the ocean; and by applying to the sea the theory which has already given a satisfactory explanation of the trade-winds, that we shall succeed in unravelling the subject under consideration. It will thus, in my opinion, be equally possible to conceive how currents of very inconsiderable velocity cross such immense extents of sea; how they are inflected and reflected in their course, by the coasts of continents and islands *while yet at a distance;* and how they deviate when they approach banks, such as those of the Agulhas or Newfoundland, on which there is not less than fifty-five fathoms of water!

SEA OF WEEDS *(Varec)*.—Among the phenomena of the ocean, which, although so long known to us, may yet become the subject of curious investigation, I should be inclined to place that of the *Weedy Sea*, or the *Sea of Wrack*.

These names are applied to a portion of the Atlantic Ocean, situated to the west of the Azores. It is, on an average, from forty to fifty leagues in width; its extent in latitude is 25°: and the space which it occupies is nearly equal in area to the surface of France*. It is entirely covered with plants *(Fucus natans)*. The Portuguese call it *Mar de Sargasso;* Oviedo, *Praderias* (Prairies) *de Yerva*. In 1492 the companions of Christopher Columbus were greatly alarmed by it,—they conceived that they had reached the remotest limits of the navigable ocean, and expected to be stopped by the weed, as their fabulous St. Barandan had formerly been by the ice of the polar regions.

By examining a multitude of observations on the subject, deposited in the archives of the English Admiralty, in order to determine the limits of the Sea of *Sargasso*, Major Rennell found that this great bank of fucus had undergone no change of place between the years 1776 and 1819, either in longitude or latitude. This remarkable permanency of position M. von Humboldt has shown to have existed so far back as the end of the fifteenth century, by discussing the observations of Columbus.

Three different explanations have been advanced to account for the existence of *Fucus natans* in this sea. Some are of opinion that there are, in these latitudes, numerous banks at the bottom of the ocean on which the fuci grow, and from which they are accidentally detached; others, that these plants vegetate, and develop themselves even on the surface of the water; but the opinion most generally received is, that the Sea of Weeds is merely the site where the Gulf-stream continually deposits the plants with which it is loaded on issuing from the Gulf of Mexico.

This last-mentioned hypothesis has been adopted by Major Rennell, although it is very far from explaining why a great proportion of the floating weeds in the Sea of Sargasso are, instead of being faded or decayed, in a state of great freshness. Indeed, English navigators never fail, when they speak of these regions, to mention the *fresh weed*, and *the weed much decayed*. Christopher Columbus himself, as M. von Humboldt has remarked, was likewise much struck with the mixture of *yerba muy vieja y otra muy fresca*.

The floating fuci of the Sea of Sargasso are always destitute of roots and fruit. If we suppose them to be developed in the same region where they are found, we must consider them to be, as M. Meyen has done, similar to fresh-water algæ, many of which multiply only by new branches. It will likewise remain to be explained by what means it is that the waters over such a great extent of sea escape so completely from the action of winds and currents, that centuries have not been sufficient to disperse the plants which were found collected there at the end of the fifteenth century, when the galleys of Columbus ploughed through them for the first time.

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* *It* would therefore be 140 to 170 miles from east to west, and about 1700 miles from north to south; an area of about 264,000 square miles.—ED.

It doubtless appears more natural to suppose, that, in proportion as the winds and currents drive the floating fuci beyond the ordinary limits of the Sea of Sargasso, their places at the surface are occupied by others detached from the bottom. According to this hypothesis the fuci are stationary in appearance only; the sea would always appear alike covered over the region which produces them, yet the individuals would be continually regenerated.

What, then, is necessary at the present time to throw light on this curious point in terrestrial physics? A few experiments, which, though extremely simple, are still wanting to science,—soundings, sufficiently deep, made along the borders, and towards the centre, of the Sea of Sargasso.

[To be concluded in our next.]

UNIVERSITY OF LONDON.

This University is at length constituted by Royal Charter, of which the following is a verbatim Copy:—

William the Fourth, by the grace of God, of the United Kingdom of Great Britain and Ireland, King, Defender of the Faith, to all to whom these presents shall come, greeting: Whereas, we have deemed it to be the duty of our Royal office, for the advancement of religion and morality, and the promotion of useful knowledge, to hold forth to all classes and denominations of our faithful subjects, without any distinction whatsoever, an encouragement for pursuing a regular and liberal course of education; and considering that many persons do prosecute or complete their studies, both in the metropolis and in other parts of our United Kingdom, to whom it is expedient that there should be offered such facilities, and on whom it is just that there should be conferred such distinctions and rewards as may incline them to persevere in these their laudable pursuits: Now know ye, that for the purpose of ascertaining, by means of examination, the persons who have acquired proficiency in literature, science, and art, by the pursuit of such course of education, and of rewarding them by academical degrees, as evidence of their respective attainments, and marks of honour proportioned thereunto, we do, by virtue of our prerogative Royal, and of our especial grace, certain knowledge, and mere motion, by these presents, for us, our heirs, and successors, will grant, declare, and constitute,—

Our right trusty and well-beloved cousin, William Cavendish, Earl of Burlington.

The Right Rev. Father in God, Edward, Lord Bishop of Durham.

The Right Rev. Father in God, William, Lord Bishop of Chichester.

Our right trusty and well-beloved Councillor, Henry Baron Brougham and Vaux, and

Our trusty and well-beloved George Biddell Airy, Esq., our Astronomer Royal, and Fellow of the Royal Society.

Andrew Amos, Esq., Barrister-at-Law.

Thomas Arnold, Doctor in Divinity.

John Austin, Esq., Barrister-at-Law.

Neil Arnott, Esq., Doctor in Medicine.

John Bacot, Esq.

Francis Beaufort, Esq., Captain of our Royal Navy, Hydrographer of the Admiralty, and Member of the Naval Society.

Archibald Billing, Esq., Doctor in Medicine, and Fellow of the Royal College of Physicians.

William Thomas Brande, Esq., Vice-President of the Royal Society.

James Clarke, Esq., Doctor in Medicine, Fellow of the College of Physicians, and of the Royal Society.

Philip Cecil Crampton, Esq., Doctor of Civil Law, Fellow of the Royal Society, and our Surgeon-General in Ireland.

John Dalton, Esq., Doctor of Civil Law, and Fellow of the Royal Society.

William Empson, Esq., Barrister-at-Law, Professor of General Polity and the Laws of England at the East India College.

Michael Faraday, Esq., Doctor of Civil Law, Fellow of the Royal Society.

Sir Stephen Love Hammick, Bart., Doctor in Medicine, Fellow of the Royal College of Physicians, and of the Royal Society.

John Stevens Henslow, Clerk, Master of Arts, Professor of Botany in the University of Cambridge.

Cornelius Hewett, Esq., Doctor in Medicine, and Downing Professor of Medicine in the University of Cambridge.

Thomas Hodgkin, Esq., Doctor in Medicine.

Francis Kiernan, Esq.

John George Shaw Lefevre, Esq., Fellow of the Royal Society.

John William Lubbock, Esq., Vice-President and Treasurer of the Royal Society.

Sir James M'Grigor, Bart., Doctor in Medicine, Doctor of Civil Law, Fellow of the Royal Society, Fellow of the College of Physicians, one of our Physicians Extraordinary, and Director-General of the Army Medical Board.

Richard Rainy Pennington, Esq.

Jones Quain, Esq., Doctor in Medicine.

John Rideout, Esq.

Peter Mark Roget, Esq., Doctor in Medicine, Secretary of the Royal Society.

Nassau William Senior, Esq., one of the Masters of our High Court of Chancery, and Fellow of the Royal Society.

Joseph Henry Gerrard, Doctor of Laws, Principal of the Bristol College.

Richard Sheepshanks, Clerk, Fellow of the Royal Society.

John Sims, Esq., Doctor in Medicine.

Cornop Thirlwall, Clerk, Fellow of Trinity College, Cambridge.

James Walker, Esq., Fellow of the Royal Society, and

Henry Warburton, Esq., Member of the Commons' House of Parliament, and Fellow of the Royal Society,—

During our Royal will and pleasure, and all persons whom we may hereafter appoint to be Chancellor, Vice-Chancellor, or Fellows, as hereinafter mentioned, one body *politic* and corporate, by the name of the *University* of London, by which name such *body politic* shall have perpetual succession, and shall have a common seal, and shall by the same name sue and be sued, implead and be impleaded, and answer and be answered unto in every Court of us, our heirs, and successors. And we do hereby will and ordain, that by the same name they and their successors shall be able and capable in law to take, purchase, and hold to them and their successors any goods, chattels, or personal property whatsoever, and shall also be able and capable in law, notwithstanding the statutes of mortmain, to take, purchase, and hold to them and their successors, not only all such lands, buildings, hereditaments, and possessions, as may be from time to time exclusively used and occupied for the immediate purposes of the said University, but also any other lands, buildings, hereditaments, and possessions whatsoever, situate within our United Kingdom of Great Britain and Ireland, not exceeding the annual value of 10,000*l.*; such annual value to be calculated and ascertained at the period of taking, purchasing, or acquiring the same; and that they and their successors shall be able and capable in law to grant, demise, alien, or otherwise dispose of, all or any of the property, real or personal, belonging to the said University, and also to do all other matters incidental or appertaining to a body corporate. And we do hereby further will and ordain that the said body politic and corporate shall consist of Chancellor, one Vice-Chancellor, and such number of Fellows or Members of the Senate as we shall from time to time appoint under our sign manual; and that our right trusty and right well-beloved cousin the aforesaid William Cavendish, Earl of Burlington, be the first Chancellor, John William Lubbock, Esq., the Vice-Chancellor, and the aforesaid Edward Lord Bishop of Durham, William Lord Bishop of Chichester, Henry Baron Brougham and Vaux, George Biddell Airy, Andrew Amos, Thomas Arnold, John Austin, Neil Arnott, John Bacon, Francis Beaufort, Archibald Billing, William Thomas Brande, James Clarke, Philip Cecil Crampton, John Dalton, William Empson, Michael Faraday, Sir Stephen Love Hammick, John Stevens Henslow, Cornelius Hewett, Thomas Hodgkin, Francis Kiernan, John George Shaw Lefevre, John William Lubbock, Sir James M'Grigor, Richard Rainy Pennington, Jones Quain, John Rideout, Peter Mark Roget, Nassau William Senior, Joseph Henry Gerrard, Richard Sheepshanks, John Sims, Cornop Thirlwall, James Walker, and Henry Warburton, be the first Fellows and Members of the Senate thereof. That whenever a vacancy shall occur in the office of Chancellor of the said University, either by death, resignation, or otherwise, we will, under our sign manual, nominate a fit and proper person to be

Chancellor instead of the Chancellor occasioning such vacancy. That the office of Vice-Chancellor of the said University shall be an annual office; and the Vice-Chancellor hereinbefore named shall, at the expiration of one year from the 1st of July, 1837, go out of office, and the said Fellows or Members of the Senate shall, at the meeting to be holden by them for that purpose, on some day within a month before the expiration of the tenure of the said office, of which due notice shall be given, elect one other fit and proper person to be the Vice-Chancellor of the said University, and so from time to time annually; or, in case of the death, resignation, or other avoidance of such Vice-Chancellor, before the expiration of his year of office, shall, at a meeting to be holden by them for that purpose as soon as conveniently may be, of which due notice shall be given, elect some other fit and proper person to be Vice-Chancellor for the remainder of the year in which such death, resignation, or other avoidance shall happen; such person to be chosen from among themselves by the major part of the Fellows present at such meeting, and to be approved of by the Chancellor of the said University for the time being.

That we reserve to ourselves to be the visitor of the said University of London, with authority to do all things which pertain to visitors, as often as to us shall seem meet.

That the Chancellor, Vice-Chancellor, and Fellows, for the time being, shall have the entire management of, and superintendence over, the affairs, concerns, and property of the said University; and in all cases unprovided for by this our charter, it shall be lawful for the Chancellor, Vice-Chancellor, and Fellows, to act in such manner as shall appear to them best calculated to promote the purposes intended by the said University; and the said Chancellor, Vice-Chancellor, and Fellows, shall have full power from time to time to make, and also to alter any by-laws and regulations (so as the same be not repugnant to the laws of our realm, or to the general objects and provisions of this our charter) touching the examinations for degrees, and the granting of the same, and touching the mode and time of convening the meetings of the Chancellor, Vice-Chancellor, and Fellows, and in general touching all other matters whatsoever regarding the said University; and all such by-laws and regulations, when reduced into writing, and after the common seal of the said University shall have been affixed thereto, shall be binding upon all persons members thereof, and all candidates for degrees to be conferred by the same; all such by-laws and regulations having been first submitted to one of our principal Secretaries of State, and approved of and countersigned by him.

That all questions which shall come before the Chancellor, Vice-Chancellor, and Fellows, shall be decided by a majority of the members present; and the chairman at any such meeting shall have a vote, and in case of an equality of votes, a second or casting vote.

That no question shall be decided at any meeting unless the Chancellor, or Vice-Chancellor, and five Fellows, or, in the absence of the Chancellor and Vice-Chancellor, unless six Fellows at the least, shall be present at the time of such decision.

That at every meeting of the Chancellor, Vice-Chancellor, and Fellows, the Chancellor, or in his absence the Vice-Chancellor, shall preside as chairman, or in the absence of both, a chairman shall be chosen by the members present, or the major part of them.

That the said Chancellor, Vice-Chancellor, and Fellows, for the time being, shall have full power from time to time to appoint, and, as they shall see occasion, to remove all examiners, officers, and servants of the said University.

That once, at least, in every year, the said Chancellor, Vice-Chancellor, and Fellows, shall cause to be held an examination of candidates for degrees; and on every such examination the candidates shall be examined either by examiners appointed for the purpose from among the Fellows by the said Chancellor, Vice-Chancellor, and Fellows, or by other examiners so to be appointed; and that on every such examination the candidates shall be examined in as many branches of general knowledge as the said Chancellor, Vice-Chancellor, and Fellows, shall consider the most fitting subjects of such examination. And whereas it is expedient to extend the benefits of colleges and establishments already instituted, or which may be hereafter instituted, for the promotion of literature, science, and art, whether incorporated or not incorporated, by connecting them for such purposes with the University created by this our Royal charter—We do hereby further will and ordain, that all persons shall be admitted as candidates for the respective degrees of Bachelor of Arts, Master of Arts, Bachelor of Laws, or Doctor of Laws, to be conferred by the said University of London, on presenting to the said Chancellor, Vice-Chancellor, and Fellows, a certificate from any of the institutions hereinafter mentioned, to the effect that such candidate has completed the course of instruction which the said Chancellor, Vice-Chancellor, and Fellows, by regulation in that behalf shall determine.

That such certificates as aforesaid may be granted from our college called University College, or from our College called King's College, both situate in London, or from such other institution, corporate or unincorporate, as now is, or hereafter shall be, established for the purposes of education, whether in the metropolis or elsewhere within our United Kingdom, and as we, under our sign manual, shall hereafter authorize to issue such certificates.

And for the purpose of granting the degrees of Bachelor of Medicine, and Doctor of Medicine, and for the improvement of medical education in all its branches, as well in medicine as in surgery, midwifery, and pharmacy—We do further hereby will and ordain that the said Chancellor, Vice-Chancellor, and Fellows, shall from time to time report to one of our principal Secretaries of State, what appear to them to be the medical institutions and schools, whether corporate or unincorporated, in this our metropolis, or in other parts of our United Kingdom, from which either singly or jointly with other medical institutions and schools in the country or in foreign parts, it may be fit and expedient in the judgment of the said Chancellor, Vice-Chancellor, and Fellows, to admit candidates for medical degrees, and on approval of such report by our said Secretary of State, shall admit all persons as candidates for the respective degrees of Bachelor of Medicine and Doctor of Medicine, to be conferred by the said University, on presenting to the said Chancellor, Vice-Chancellor, and Fellows, a certificate from any such institution or school, to the effect that such candidate has completed the course of instruction which the said Chancellor, Vice-Chancellor, and Fellows, from time to time, and with the approval of one of our principal Secretaries of State, to vary, alter, and amend any such reports, by striking out any of the said institutions or schools included therein, or by adding others thereunto.

That the said Chancellor, Vice-Chancellor, and Fellows, shall have power, after examination, to confer the several degrees of Bachelor of Arts, Master of Arts, Bachelor of Laws, Doctor of Laws, Bachelor of Medicine, Doctor of Medicine, and to examine for medical degrees in the four branches of medicine, surgery, midwifery, and pharmacy, and that such reasonable fees shall be charged for the degrees so conferred, as the said Chancellor, Vice-Chancellor, and Fellows, with the approbation of the Commissioners of our Treasury, shall from time to time direct; and such fees shall be carried to one general fee fund for the payment of the expenses of the said University, under the directions and regulations of the Commissioners of our Treasury, to whom the accounts of income and expenditure of the said University shall once in every year be submitted, which accounts shall be subject to such examination and audit as the said Commissioners may direct.

That at the conclusion of every examination of the candidates, the examiners shall declare the name of every candidate whom they shall have deemed to be entitled to any of the said degrees, and the departments of knowledge in which his proficiency shall have been evinced, and also his proficiency in relation to that of other candidates, and he shall receive from the said Chancellor a certificate, under the seal of the said University of London, and signed by the said Chancellor, in which the particulars so declared shall be stated.

Provided always, that all by-laws and regulations made from time to time touching the examinations of candidates, and granting of degrees, shall be submited for the consideration of one of our principal Secretaries of State, to be approved of by him.

And, lastly, we do hereby for us, our heirs and successors, grant and declare that these our letters patent, or the enrolment or exemplification thereof, shall be in and by all things valid and effectual in law, according to the true intent and meaning of the same, and shall be construed and adjudged in the most favourable and beneficial sense for the best advantage of the said University, as well in all courts as elsewhere, notwithstanding any non-recital, misrecital, uncertainty, or imperfection, in these our letters patent.

In witness whereof, we have caused these our letters to be made patent. Witness ourself, at our Palace of Westminster, the 28th day of November, in the seventh year of our reign.

By Writ of Privy Seal,
EDMUNDS.

MISCELLANIES.

Nomenclature of Comets.

designation of comets by the names
en, is now established by custom.
his practice may be useful in ex-
g the zeal of astronomers, it may
ell to adhere to it. One condition,
ver, seems indispensable,—that the
es shall be constantly chosen by an
iable rule, which shall be free from
ersonal and national prejudices.
ther or not such a rule has been
ved, with strict justice, up to the
nt time, may be learnt from the
ving statement:—
ree periodical comets are now
n;—the comet whose revolu-
requires 76 years: the comet of
rs $\frac{3}{10}$; and that of 6 years $\frac{3}{4}$. The
bears the name of *Halley;* the
d, that of *Encke;* the third, that
iela. These three appellations, evi-
ly do not proceed from the same rule.
each case of a periodical comet,
may be distinguished,—the astro-
er who was the first to observe it;
astronomer who was likewise the
to ascertain, by the means of its
bolic elements, that it had previous-
ppeared; and finally, the astrono-
who, by the study of its elliptic
ents, calculated the time of its
ution. We may give the prefer-
, according to our own peculiar
s, either to a calculator, or to the
rver; but the decision once made,
uld be unjust not to adhere to it.
us now, leaving Halley out of the
tion he having been the first who
attention to periodical comets, exa-
on what grounds the comet dis-
red by M. Pons, on the 26th No-
ber, 1818, has generally taken the
e of Hr. Encke. It is, indisputably,
the reason, that the celebrated
nomer of Berlin was the first to
late its elliptic elements; it is, that
calculation was considered more
rtant, more difficult, and more
rving of grateful recollection, than
discovery: but if this be true of
comet of 1818, it should not be
idered false when applied to the
t of 1826. The discoverer of this
*whatever may be his rank or posi*tion in the world, ought not to be more favoured than M. Pons, the *discoverer* of the comet of 1818. He ought, in justice, to give up the honour to the *calculator.* For the same reason that the comet of the short period is called that of *Encke,* (and for my part, I consider this appellation highly proper,) the comet of 6$\frac{3}{4}$ years, should bear the name of GAMBART[*]. Those who may persist in calling it the comet of *Biela,* will evidently have two weights and two measures, for the Austrian officer, like M. Pons, *simply saw* the comet before any other observer—he followed its march among the constellations, but he *calculated* neither its parabolic, nor its elliptic orbit.—ARAGO.

Honour due, and granted.—Mr. Francis Baily.

THE circumstances of the recent election of a corresponding member of the Section of Astronomy in the *Académie des Sciences,* of Paris, cannot fail to be highly gratifying to our scientific countrymen.

At a meeting of the Academy on the 14th of November last, the Astronomical Section presented the following list of persons, whose services and reputation in astronomical science the Section considered sufficient to render them worthy of the distinction of filling the vacancy produced by the death of Mr. Pond:—

1. Baily	*London.*
2. Dunlop . . .	*Paramatta.*
3. Carlini . . .	*Milan.*
4. Littrow . . .	*Vienna.*
5. Hansen . . .	*Gotha.*

Two of these, and the first on the list, it will be observed, are Englishmen.

On the 21st of November, the Academy proceeded to the election. There were forty-two members present who voted, and the result was—

34	for	Baily.
7	for	Carlini.
1	white ball.	
—		
42		

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[*] The acute and accomplished Director of the Observatory at Marseilles, recently deceased.

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Mr. Baily was therefore declared a correspondent member of the Academy. The scientific rank of England, is, therefore, still maintained in the Astronomical Section of this distinguished body.

Doubts of the Efficiency of Fusible Discs on Steam-boilers.

The use of discs of fusible metal, as a means of preventing explosion in steam-boilers, though little practised in this country, was considered by our neighbours on the continent as one of the most efficacious. It received the approbation of the *Académie des Sciences*, and the French government enforced it by a law, in which the proportions of the alloy, the dimensions of the discs, &c. were minutely prescribed. Experience has since shaken the confidence reposed on this means, and a commission which has recently sat at Toulon upon the subject, have decided, that, in their opinion, these discs furnish no preservation against explosions, but are a source of real danger. The statement which this commission laid before the Government, has induced the latter to take into consideration the propriety, not only of suspending the present regulations which enforce their use, but, in the new ones about to be issued, of forbidding the use of these discs altogether in every kind of steam-generator, whether in navigation or manufactures.

The proposal for the adoption of this means of security, having proceeded from the Academy, the Government have thought proper to call upon that body for an examination of the objections which have been raised, and for a thorough discussion of this important question.

The academy have appointed MM. Arago, Dulong, Dupin, D'Arcet, and Seguier, to report upon the subject.

Proportion of Infants Still-born.

From an examination of documents, selected as favourable to a correct result, it would appear, that the number of children still-born, bears the following proportion in the undermentioned places.

Strasburg		1 in 11 births.
Hamburg		1 „ 15 —
Amsterdam		1 „ 17 —
Dresden		1 „ 17 —
Paris		1 „ 19 —
Berlin		1 „ 20 —
Vienna		1 in 24 births.
London		1 „ 27 —
Prussian monarchy		1 „ 29 —
Brunswick		1 „ 33 —
Stockholm		1 „ 36 —
Belgium	in the towns	1 „ 20·4 —
	in the country	1 „ 38·2 —
Geneva		1 „ 15·5 —
Issoudun		1 „ 28·0 —
Sens		1 „ 19 —

The differences that may be remarked in the numbers furnished by the various countries and places are, perhaps, not quite so great as they would appear at the first glance. For actually, the still-born entered in the registers under that name, may be divided into two classes, one of which would contain those which are without life at the moment of birth, and the other, those which die within the three days allowed for the declaration of the death to be made. The influence of the law, of municipal regulations, and of the negligence of poor and obscure families, has a great effect upon the second class, which must be more numerous in the towns and the great seats of manufacture, than in the villages and in the country.

The town of Issoudun, situated in the centre of France, free from the agitations and changes of the manufacturing districts, of an average size and population, appeared well adapted to afford a mean between the cities, and the country, of France. An extract from its registers was obtained, which contains an account of all infants still-born, for thirty years. The proportion, $\frac{1}{28}$, which is the result of this research, approaches very nearly to the mean of the numbers given by M. Quetelet for the towns and villages of Belgium, and to the number corresponding to the Prussian monarchy. And until we are in possession of more accurate documents on the subject, this proportion, $\frac{1}{28}$, is, probably, the true one for the whole of France. —Demonferrand.

Paradox in graduating Circular Instruments.

In a theodolite constructed by M. Gambey of Paris, for Mr. Pentland, the minister despatched by the English government to Bolivia, the graduation is said to have been accurately and beautifully executed by a machine, in which it is not necessary *that the circle* to be graduated should have its centre *adjusted to that of the dividing-plate.* Many skilful mechanicians do not believe this to be possible; it is, however, asserted, that M. Gambey accomplishes it with invariable success, by connecting the tracing-point with an articulated train of great simplicity. The great excellence of M. Gambey's instruments results, principally, from the ingenious tools he has designed for their fabrication. The theodolite of Mr. Pentland repeats both horizontally and vertically, and such is the truth and clearness of the graduation, that, though the circles are but 6·3 in. radius, they can be read off with the greatest certainty to *five seconds.*

Chemical Rays of the Spectrum.—Mrs. Somerville, M. Arago, and Sig. Melloni.

In the report of the meeting of the *Académie des Sciences,* on the 21st of December, 1835, it was stated, That after having pointed out the remarkable parts of the experiment, by means of which Sig. Melloni proved, that the solar rays may preserve all their luminous properties, and yet, at the same time, lose all their heating powers, M. Arago remarked, that the question might be regarded from another point of view. In his opinion, it would be important to examine, if the processes employed by Sig. Melloni, or some analogous ones, would not lead to the fact of depriving the solar rays of their *chemical* effects; if, in a word, of the three properties which solar light possesses when it reaches the earth, viz. 1, that of illuminating; 2, that of heating; and 3, that of decomposing, or affecting chemical combinations, it would be possible to remove the two last, and preserve the illuminating property only.

"This experiment," continued M. Arago, "appears to me as if it would produce some curious consequences, and in the past week I had nearly yielded to the temptation of trying it. But, as it was possible that Sig. Melloni might also have thought of it, though he has not mentioned it in his memoir, it appeared to me that I ought not to pursue my intention, until after I had conferred with the learned Italian *physicien.*"

"The motives, which influenced me in 1835," observed M. Arago to-day, (17 Oct. last,) "not to anticipate Sig. *Melloni in an inquiry* so intimately connected with his beautiful discoveries, are still in operation; and I shall, therefore, abstain from mentioning some results which I have obtained on the absorption or interception of the chemical rays. The same reserve, it is clear, cannot be asked of Mad. Somerville. I, therefore, see no reason to refuse to the interesting experiments of a person so eminently distinguished, all the publicity which the meetings and reports of the academy can confer."

M. Arago then communicated the following extract of a letter, addressed to him by Mrs. Somerville, detailing some *experiments on the transmission of the chemical rays of the solar spectrum through various media.*

"I used in these experiments some chlorate of silver, of the most perfect purity and whiteness, which Mr. Faraday had the kindness to prepare for me. It was fluid, and could be laid very evenly on paper. Although this substance is extremely sensible to the action of the chemical rays, yet as there are no precise means for measuring the changes of colour produced by this action, there is a difficulty in accurately describing results, when it is desired to compare tints which are nearly the same; but the results which I now offer, are chosen from such as do not admit of any doubt.

"A small piece of glass, of a very pale green, perfectly transparent, and less than $\frac{1}{20}$ in. in thickness, did not permit the passage of any chemical ray whatever; after an exposition for half an hour to the sun when very hot, the chlorate of silver, which was placed behind the glass, exhibited no change of colour.

"I repeated this experiment with several different green-coloured glasses, of various tints and thicknesses. I found them all nearly impermeable by the chemical rays, even when they were submitted a much longer time to the solar influence. As Sig. Melloni has already discovered that glass of this colour intercepted the most refrangible of the calorific rays, we shall be induced, by considering his results with mine, to conclude, that this glass has the property of intercepting entirely the most refrangible part of the solar spectrum.

"Plates of deep-green mica are also nearly impermeable by the chemical rays; but when the plates are very thin, and the solar action is very much prolonged, it is evident they do not intercept the rays entirely. I fixed, by means of soft wax, on a sheet of paper prepared with chlorate of silver, a plate of pale-green mica from Vesuvius, the thickness of which did not exceed $\frac{1}{30}$in. I exposed the whole to the rays of a hot sun; at the end of some time, the mica was removed, and I found that the portion of paper which it had covered had lost nothing of its whiteness, while all the other part had acquired a deep-brown colour.

"A similar experiment was made with thin plates of white mica: six plates of ordinary white mica being superposed, did not intercept the chemical rays; the chlorate of silver which they covered, becoming, at the end of an hour's exposition to the sun, of a deep-brown. The same result was obtained, with a single, but much thicker, plate of white mica. This substance, therefore, appears not to offer any obstacle to the passage of the calorific [chemical ?] rays.

"These experiments disposed me, in the first instance to believe, that all green substances possessed the same property, but I was not long in finding that I had been too hasty in generalizing results; in fact, having submitted to the same tests a large emerald, the green colour of which was very fine, but not very deep, and whose thickness was at least $\frac{1}{33}$ in. I found that the chemical rays were transmitted without difficulty; therefore, the matter which colours the emerald green has no action on the chemical rays, while that which imparts the same colour to glass and to mica, exerts a decided influence upon them.

"Rock-salt, as there was every reason to expect, possesses, in a very high degree, the property of transmitting the chemical rays. Glass, coloured violet by manganese, and the deep-blue kind which is used in finger-glasses for the table, transmit these rays rapidly. The change of chlorate of silver by the solar action takes place quickly, though a plate of deep-blue glass, $\frac{1}{4}$in. thick, be interposed.

"Among the various substances that I have exposed in these experiments, rock-salt, white, blue, and violet glass, are those which present the maximum of permeability to the chemical rays; green glass and green mica possess the minimum. Other bodies have this property in intermediate degrees; it varies in some, even where the colour is nearly the same; thus, deep-red glass scarcely permits the passage of any chemical rays, while the garnet, equally deep-red, transmits nearly the whole of them. The white topaz, as well as the blue, the pale-blue beryl, cyanite, heavy spar, the amethyst, and various other substances, transmit with great facility the chemical eays; but the yellow beryl may be said to permit none to pass; and the brown tourmalin, as well as the green one, has so little permeability, that I failed in the different attempts which I made to polarize the rays in question, although I am of opinion, that the thing would not be absolutely impossible with plates prepared much thinner than those which I employed. I intend to resume, shortly, this train of experiments."

Goethe and De Candolle.

"GOETHE, the greatest poet and the most distinguished philosopher of Germany, was afflicted, about the middle of his career, with severe depression of spirits; the social and civil disorders of France induced such moral agony, that, during his attack of hypochondria, he withdrew to a retreat in Italy, to pass the time among the magnificent gardens which embellish that country. The sympathies of the poet were at first scarcely excited by the contemplation of these exquisite scenes; but by degrees the flowers flattered his senses, and as a means of recreation, his soul indulged in their gorgeous beauties. At the same time that their variety called forth his admiration, and he distinguished their differences, he was struck with their analogies. He believed in their translation of form; this intuitive poetry consoled him, and became such an ever-springing source of pleasure, that the conception of a work on Vegetable Physiology was a consequence. Goethe committed his inspirations to writing, and gave them the form of aphorisms, intending them for his private use only. At a later period, in 1790, he sent his work into the world, under the title of an *Essay on the Metamorphosis of Flowers*.

"The human intellect was not then prepared, in the slightest degree, to receive, under so anomalous a form, this revelation of relations; and at the present day, how many botanists are there who are arrived at this point?

"Three years passed away, and Goethe resumed his wonted occupations as poet and philosopher; but he felt so little confidence as a naturalist, that he frequently made secret incursions into the regions of zoology and of anatomy, to search for proofs and analogies to support and justify the law of harmony which he had discovered in the vegetable tribes, for he had faith in his own convictions, and in the tardy justice of posterity.

"Twenty-seven years after Goethe had been left in the rear as a naturalist, or rather forgotten under the title, his day of triumph arrived. De Candolle appeared with the rich booty he had collected during his admirable researches into the teratology of vegetables. De Candolle, who had passed through every grade of botanical knowledge, adopted, under a slightly-different form, the notions of the great poet on the analogy of organization, and immediately afterwards determined to visit, at Weimar, the illustrious old man who had enunciated these excellent ideal preconceptions in the science. There, cordially congratulating the philosopher, so tardily admitted to the title, he beheld in him a real naturalist, in the midst of a valuable cabinet of natural history, and occupied in observations upon living beings!"—Geoffroy Saint-Hilaire.

Grand Scientific Survey, by order of the Russian Government.

A scientific expedition for the purpose of accurately determining the long-disputed question of the levels of the Caspian and Black Seas, was to commence its operations in July last.

The government of Russia has undertaken it on the suggestion of the Imperial Academy of Sciences, at St. Petersburgh. It has approved of the plan of operations drawn up by the Academy, and munificently placed 8000*l*. at their disposal to defray the expenses.

The details of the organization of this expedition, and the selection of the scientific corps who are to carry its operations into effect, have been confided to Professor Struve, the astronomer of Dorpat. He has appointed three of his former pupils; Hr. George Fuss, assistant-astronomer of the Central Observatory, and who has given evidence of his ability by his labours in China and in the south-east of Siberia; Hr. Sabler, assistant-astronomer of the University of Dorpat; and Hr. Savicht, Professor of Mathematical Sciences in the University of Moscow. These are accompanied by a mechanician, and furnished with a complete selection of the necessary instruments. The time required for the operations is estimated at about eighteen months. The line of this vertical survey is to be conducted trigonometrically, and will be run, according to local circumstances, either between Taganrog and the mouth of the Kouma, along the Manitch, and across the steppe, or between Taman and Kisliar, following the range of the Caucasus.

Simultaneous barometrical observations, with instruments regularly compared, will be made, not only by the travellers along the line of operation, but also at Astracan and at Taganrog, by experienced and confidential persons stationed in those towns.

Mirage in Iceland.

"We have had very often the phenomenon of the mirage under observation; and, contrary to the opinion of some navigators who have visited the North, I was never able to remark that objects were elevated by it. It always appeared to me like a bright fog on the surface of the sea, which, variously modifying the bases of objects, might, I believe, in certain circumstances, so influence the illusion, as to give an idea of their being elevated by the phenomenon."—Robert. *Letter from Iceland, July,* 1836.

When this extract was read in the *Académie des Sciences*, M. Libri remarked, that it could not be affirmed, as M. Robert appeared to do, that objects were not elevated by the mirage, for this phenomenon, which takes place under very different circumstances, produces very varied effects. In the sandy countries of the south, lakes and sheets of water are seen during the mirage, while in the north and at sea, it is very distant objects only, and even those below the horizon, that are visible. As to whether these objects shall be seen elevated, more or less, by the effect of of the mirage, that will principally depend upon local circumstances, and the state of the atmospheric strata through which the rays must pass which emanate from the objects affected by the phenomenon.

Extemporaneous malleable Platinum.

"THE process of Wollaston for the manufacture of platinum is made use of by those persons only who manufacture this metal for commerce. Chemists never prepare malleable platinum for the uses of the laboratory, and the process is described in no public course of instruction. Hr. Liebig is, I believe, the only person who in his lectures teaches it. Though the mode he follows is exactly, in every respect, that of Wollaston, and though it presents nothing new in a scientific point of view, I nevertheless am of opinion that I shall perform a useful and agreeable office to chemists, in directing their attention to a process far too much neglected, and so easy of execution, that one may say there is really no operation whatever more simple, or more expeditious, than that of making malleable platinum in the following little apparatus.

"It is a hollow cylinder, slightly conical, one end of which is closed by a small thick metallic plate. After having decomposed, at the lowest possible temperature, the ammoniacal muriate of platinum, the froth produced is to be removed by a wooden rod, and mixed with water; after being made into a clear paste it is to be introduced into the cylinder; then, by means of an iron piston, it is to be slightly pressed for one or two minutes: this is to be followed by the greatest possible compression. A ring of iron in which the cylinder stands, permits the easy disengagement of the bit of platinum, by striking a blow on the piston.

"The platinum, when thus removed, will be found to have already acquired great density, and a brilliant metallic aspect. It should be dried gently, and afterwards exposed for a quarter of an hour to a white heat, then be suddenly withdrawn from the crucible, and struck once with a hammer. It is to be so treated four or five times, *gradually* augmenting the number of blows.

"In less than half an hour the whole operation is finished, and is so easy, that the result is always certain.

"I offer for the examination of the Academy a spatula and a knife-blade of platinum, which I saw manufactured, *in* a *few* minutes, in the laboratory of *Hr. Liebig*, at Giessen."—PELOUSE.

Geological Hebrew.

IN sawing through a block of marble, *(primitive limestone,)* which had been obtained from a quarry in Montgomery county, Pennsylvania, there was found in the interior a cavity filled with black pulverulent matter, which was denominated *primitive carbon.* When this was removed, the bottom of the cavity, which was flat, presented several lines in relief, which portrayed, distinctly, two letters of the Hebrew alphabet! It appears that the attention of American naturalists has been strongly directed to this discovery. Mr. Browne, Professor of Geology at Philadelphia, and the present possessor of the block, has offered to send it to Paris for the examination of geologists.

Motion of Water on Heated Surfaces.

PHYSICIENS have long remarked the phenomenon which water presents when thrown upon a metallic surface heated to a very high temperature. If a small quantity of water be thrown into a crucible of platinum at nearly a white heat, for instance, the drops will be seen to be tossed about from one side to the other of the crucible, or to spin round on themselves, but always continuing a very long time, though their evaporation would have been rapid, had the metal been less hot. M. Baudrimont has concluded, from experiments made with a view to explain the phenomenon, that the drops of water so moving at the bottom of an incandescent crucible, have a temperature much below that of ebullition, and that it never exceeds 122° Fahr. M. Laurent, in repeating these experiments, obtained very different results; according to him, the water, notwithstanding the slowness with which it evaporates, has a temperature approaching very nearly to 212°. M. Laurent also does not admit, that the drops, during the whole time of their existence, are separated from the bottom of the crucible by a thin sheet of vapour; he believes that, on the contrary, they leap continually, and that at each of these bounds they touch the incandescent surface, though for a very short time only. During these movements they present a singular appearance; their outline, instead of being circular, has projections more or less in number, but always of an even number.

"It is in the following way," says

M. Laurent, "that the generation of these forms may be understood. Let us suppose an elastic circle suddenly compressed in two points, diametrically opposed to each other; it will take the form of an ellipse; but if the compressing cause be suddenly annihilated, the ellipse, by its elasticity, will return to the previous circular form, and, by virtue of the velocity acquired, will not stop there, but proceed to form a second ellipse, the greater axis of which will be perpendicular to that of the first. Thus there will succeed a series of oscillations which will give ellipses whose greater axes are alternatively perpendicular to each other, and, if the movement be very rapid, the impression of a previous ellipse will remain on the retina, when that of a succeeding one is produced upon it, and the two images will be superposed in such a manner, that they will exhibit a cross with rounded ends, or a wheel with four teeth. If the images of the two extreme ellipses, and that of their mean circle, be seen at the same time, a star of eight points will be produced, &c. It is easy from this to see why the number of projections of the figure should always be of an even number.

"That which has been said of a circle will hold of a sphere, if it be elastic, as is the water-drop on the floor of an incandescent crucible.

"I convinced myself that these forms were owing to vibratory motion, by the following experiment. I placed in a porcelain cup about an ounce of mercury; I set the cup on an elastic board, which I made to vibrate with a violin-bow. The mercury presented immediately the same figures as the water in the heated crucible."

Instantaneous Calculation of Areas.

M. Gaetano Caïro has invented an instrument, to which he has given the name of *Tachymeter (rapid measurer)*. Its object is to give the area of plane surfaces bounded by any outline whatever, without the necessity of any arithmetical operation.

There are several means more or less rapid for ascertaining the areas of plane surfaces, among which that of the *planimeter* of MM. Oppikofer and Ernst has the remarkable property, that nothing more is necessary than to draw a point over the outline of any figure whatever, and the superficial content of it is marked immediately upon a dial-plate by an index.

M. Caïro, who has also devoted himself to reduce the calculation of surfaces of land, &c. to a purely graphic operation, substitutes for the figure proposed a number of trapeziums, having their bases parallel, and their height constant. This latter quantity is taken as the unit, so that if the mean bases are successively measured, the number of linear units contained in the sum of all these bases expresses that of the superficial units contained in the figure proposed.

For a very long period it has been customary to calculate approximatively the area of an irregular curve, by means of equidistant ordinates taken so near to each other that the portions of the curve intercepted by them may be considered, without any sensible error, to be short right lines. A much greater degree of precision is, however, attained by considering the given curve, on the contrary, as an assemblage of small parabolic lines, in which three points are known. Legendre, in his *Exercices de calcul intégral*, has, in certain respects, improved this method. It is also given in several other modern works. But in the operations of the most extensive surveying, even in the most important of them, the former process may be safely employed; and it is this which M. Caïro uses.

Although, in general, the area of any irregular figure bounded by a curved line, may be easily obtained by means of the tachymeter, yet when the space to be measured is any right-lined polygon, it is desirable to select a mode of decomposition which will lead to the end desired in the most simple and convenient manner. Now, in these cases, the instrument of M. Caïro, even following the process which he has himself described, abridges but very little the ordinary method of finding the area of a triangle, rectangle, &c. the bases and heights being known.

Finally, though the tachymeter does not give, under any circumstances, the area of a plane figure with the same celerity as the instrument of MM. Oppikofer and Ernst, it is, however, of a very simple construction, and it may be advantageously substituted for the graphical methods used in surveying.—Puissant.

Height of Waves at Sea.

The following observations on the height of waves in the open sea, were made during a voyage from Dieppe to Newfoundland, in the Spring of the present year. Though evidently, but approximations, we give them as an instance of the extensive and valuable effects which scientific circulars, of the nature of that* compiled by M. Arago in the *Annuaire* of this year, are calculated to produce. Pointing out the untrodden fields of nature, and describing the links still wanting to complete the chain of scientific investigation, they urge observation in the right direction; and suggesting, at the same time, the mode of operation, they render its exercise easy and inviting.

"As we had not the dip-sector described by M. Arago, we could not use that means of measuring the height of waves; we, therefore, proceeded as follows. On the 26th of March, the day before a gale of wind, there being a heavy sea, M. Aigremont ascended the mast, and attempted to place his eye in a line which should touch the crests of two waves. This he found difficult to do. He then sought to refer the crest of a wave to the horizon, at the moment when the vessel was in the trough. The mean of several observations of this kind, gave 18½ feet for the height of the waves. On the 29th, in the neighbourhood of *La petite Solé*, two vessels of different magnitude passed very near us during the day, running *à contre-bord*, and crossing the waves. The sea was frightful; it may sometimes be worse, but rarely. These circumstances appeared favourable for the attainment of the measurement we desired. At the times when a vessel was in a trough of the waves, a position in which it was nearly upright, we remarked a point on the mast which was in the right line with two wave-crests in the interval between it and us, taking care that our eyes were in the same right line. The altitude of such a point on the mast above the sea, was evidently the height of the waves. We estimated the size of these vessels, (and sailors come very near the truth on such occasions,) and supposing their masts to be of the usual proportions, the mean of the observations made on the larger vessel of the two, gave nearly 44 feet for the height of the waves: the mean by the smaller vessel, gave about 41 feet. This result is, of course, affected by all the errors which we may have made in the estimate of the size of the vessels, and in the height of their masts: but when similar observations shall be made on vessels of known size, and on whose masts marks shall be made which can be referred to, this mode appears to me to be the easiest, and probably, the most accurate."—Duhamel.

Comparative Intensity of the Solar Rays in different Latitudes.

The meteorological observations made during the later voyages of English navigators in the arctic regions, have excited a warm discussion among some *physiciens*, on the question, whether the solar rays thrown on the blackened bulb of a thermometer, produce a greater effect at the equator than near the poles. One party maintaining, that the heating-power increases with the latitude; the other, considering this idea to be rash and unsupported. Some observations of Sir John Herschel, communicated to the *Académie des Sciences*, on the 31st of October last, appears to restore, definitively, to the countries in the vicinity of the equator the privilege of which it has been attempted to deprive them. By means of a new instrument, the *actinometer*, Sir John Herschel found that, at the Cape, the direct thermometical effect of the solar rays was 120° Fahr. In Europe, the maximum never exceeds 85°†.—Arago.

The Weed-Sea.—Mar de Sargasso.

"Will you be kind enough to inform M. Arago, who recommended, in his Instructions‡ for the *Bonite*, an examination, "whether the *numerous* plants (*Fucus natans*) which are found in the open sea between the tropics, really

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* *Publishing in the present and other numbers of this Magazine.*

† With deference to such high authority, we do not see how these insulated experiments are *definitive* of the question; nor how they subvert the facts given in *Daniell's Meteorological Essays*, and which induce that author to observe, "From these facts, I conclude, that the power of solar radiation in the atmosphere *increases* from the equator to the poles."—Ed.

‡ See page 478.

vegetate in the middle of the Ocean, that I had the good fortune, in returning from Cayenne, some time before our second expedition to Iceland, to collect from a tuft of this very *Fucus* at 500 or 600 leagues (about 1700 to 2000 miles) from any shore, a bit of glass rounded by friction *(roulé)*. This appears to me to be evidence, that the plant which contained it came from an inhabited coast, and that the same may be said of all the rest. The bit of glass is in the last collection of rocks, &c. which I sent to the Museum; in the catalogue will be found the latitude and longitude of the spot in which the fact occurred, and which I thought it proper to obtain."—Robert. *Letter from Iceland, July*, 1836.

Passive Patriotism.

On the 4th of July last, the *Académie des Sciences* deposited in their secret archives, the model of a portable military telegraph, which may be used by night as well as by day, together with a dictionary of signals. The inventor of this machine, a M. Coulier, did not request that it should be submitted to any scientific or official examination by the Academy, but simply petitioned for the above *cryptical* deposition, in order, *that if an occasion offered for its useful employment in the service of the French armies, its construction should not be previously known to the enemy!*

Electricity of Clouds.

During the course of the summer of 1835, the far greater part of the clouds were electric, and nearly all which were so were positive. I scarcely found ten or twelve negative clouds among the whole mass which passed over my apparatus.

This year it is very different. Up to the present time (*August*) the clouds have been generally neutral, and even among those that were supposed to be electric from their ashy colour, and their scalloped and changing edges, many did not disturb the repose of my apparatus. Those which were ascertained to be electric were nearly all negative; at least all have been so that I have had an opportunity of making any observation upon since March.

This opposition in the electrical state of the clouds, in different years, will no doubt have the effect of rendering the problem to be solved far more complicated. But this is a very small matter, compared with the difficulties which the sudden or gradual changes produced by a mass of clouds present. These difficulties are such that I have little hope of solving even a few of them, compelled as I am to operate on far too confined a scale. My conducting wires are but about 90 feet above the surface of the ground, and do not penetrate it deeper than 40 feet. The apparatus is, therefore, constantly below the lower clouds, and receives the electrical influence from them only. It cannot, therefore, *interrogate* the superposed strata of clouds, to ascertain what belongs to each, and thus to obtain the necessary means for the analysis of the various phenomena which concur in the formation of tempests. Operations must be carried on upon a larger scale, in a country where lofty mountains would facilitate the fixing of the apparatus at different altitudes, so that it might indicate the electrical state of the clouds which interchange their electricities. I shall proceed to quote some observations which may be useful to those who are engaged in the same inquiries, or who may intend to be so.

My apparatus consists of a copper wire, lapped with silk, and covered with several coats of thick varnish. The higher end finishes with a tuft of platinum wire, and the lower one is also of platinum, and is inserted in a well. In the middle is a multiplicator of 3000 turns of varnished wire, or an electroscope, according to circumstances. The electricity which forms the descending current of the wire being always of the same kind as that of the cloud, a simple indication of it is always sufficient to show that of the latter.

On the 2nd, 3rd, and 8th of April last, the clouds being strongly negative, the deviation of the needle was maintained several times at 80° for four or five minutes. During the 8th, there were some great and sudden changes, which produced deviations of 90° in the opposite direction. As similar reverses are frequent at the moment of electrical discharges during storms, I do not doubt, although I did not hear them, that there were several claps of thunder. Each time that the negative deviation very suddenly increased, there followed a shower of rime or minute hail. I had previously remarked several times this coincidence with rain and a sudden electrical change in clouds. I have this year had several opportunities of making the same observation. The remainder of the month, and the whole of May, was but slightly electric. The clouds rarely induced my apparatus to *speak*.

On the 8th of June, about 4 P. M. I had a descending negative current; rain began to fall at 5. The current suffered several negative and positive changes. The rain-drops were not affected in the same manner: they constantly gave negative indications by the electroscope. On the 11th, the commencement of a storm was positive, the middle negative, and the termination became again positive.

On the 16th, I was awakened at a quarter past 2 A. M.—a storm was raging on every side. I ran to my instruments; they indicated 80°, and a descending negative current. Some large flashes of lightning scarcely produced 10° to 15° of diminution in the deviation; at half-past 2, the negative current still giving 70°, a very large flash occurred, the needle spun round on its axis, and settled at 80° on the other side; it stood there thirty seconds, and then returned to the 70°, as before the flash. The storm passed off about three-quarters past 2, the needle returned to zero, then moved to the positive side, and rested.

In the selection of instances, I shall give the preference to the one of the 21st of July, because the cloud which I observed was of a very limited extent, and excited some hope that I might ascertain the cause of these anomalies. This cloud had passed over the apparatus for a fourth of its length before any indication was produced; at this point, a descending negative current of 4° to 5° began to take place; it increased with the progress of the cloud. Suddenly, the deviation ascended to 70°. I therefore predicted that the rain had separated from the cloud, and would shortly appear; this actually took place in a few seconds after. During the shower, the current continued negative, and was not positive till the end of it.

The showers on the 29th July gave no electrical indication.

Contrary to all preceding storms, that of the 4th August, at 2 A.M., was positive at first, and negative as it continued. The needle moved gradually towards a *maximum*, a flash occurred, and the needle returned half-way; it then recommenced its ascending progress until the next flash, which reversed its motion. This progression coincided so closely with the electrical changes, that I was able to decide that they were simultaneous with the separation of the rain from the cloud. My second apparatus had its internal needle injured by the strength of the current.

Finally, the last storm, that of the 6th August, produced at least five and twenty changes. It was also negative at the commencement and positive at the end.

Though confined as the sphere is upon which I can operate in my observations, I shall continue to seize every favourable opportunity of throwing light on this obscure subject.—PELTIER.

The State assisted by Science.

THE intercourse between the French government and the *Académie des Sciences* appears to be very frequent; and the objects are of the most interesting kind. The applications of the Academy for means, for authority, for access to documents, &c. are never disregarded, and, on the other side, no branch of the administration ever ventures to decide upon a measure in which the natural and applied sciences can furnish any information, without asking for information, or soliciting advice, from the Academy. We have already given one instance at p. 484. Here is another, demanding a more extensive range of inquiry, and an extent of laborious examination and comparison which would terrify many an F.R.S. among our countrymen, and induce him to abandon his initials rather than expose himself to be so questioned and set to work.

The Minister of Finance thus writes to the Secretary of the Academy:—

"Sir,—The Commission directed by the *Ordonnance* of the King, dated the 29th March, 1836, to examine if there be any ground for altering or modifying the conditions of Article 219 of the Code of Forest-law relating to the felling of the woods of individuals, having, in the sitting of the 11th June last, taken into consideration various points in statistics, in meteorology, and administration, were of opinion that information might be usefully requested from the various ministers and public offices, from the prefects, and from the Académie des Sciences.

"The minutes of this sitting which I have before me, contain instructions to request of the Academy the solution of the following meteorological questions:

"1. Are there existing in France thermometrical observations from which it can be deduced that the temperature is constant; or, on the contrary, that it varies? Does the snow lie as long on the summits of mountains as it formerly did?

"2. From the earliest times described in history to the present, have the seasons of harvest, of fruit-time, and of the vintage, undergone any change? Have they not in some particular localities? Is rain more or less abundant?

"3. Is the fall of snow as great as during the last century? Is there any diminution in springs which can be attributed to the felling of woods?

"4. In districts which were formerly exempt from hail, are the effects of it injurious at the present day?

"5. Has it been observed that the frequency of storms has increased since the destruction of the forests?

"6. Have inundations of rivers been greater and more sudden than before the Revolution?

"7. Has the direction of prevailing winds been changed by clearing away the woods? Are they more violent? more destructive? more insalubrious?

"8. Are the beds of rivers sensibly elevated? If they are, what is the annual amount of their rise?

"I shall be obliged by your submitting these various inquiries to the attention of the Academy and its correspondents in the departments, and to communicate the result to me."

The Academy appointed a Commission to carry the request of the minister into effect. This commission consists of MM. Dulong, Arago, Gay-Lussac, Silvestre, Girard, Mirbel, and Costaz.

Easy solidification of Carbonic Acid.—Extraordinary artificial Cold.

After having examined, in succession, the various properties of liquid carbonic acid,—its *specific gravity*, which is so variable that between 32° and 86° it runs through all the scale of densities, from that of water to that of the ethers,—its *dilatability*, four times greater than that of air itself,—the *pressure*, and the *weight* of its vapour,—its *capillarity*, and, above all, its *compressibility*, a thousand times greater than that of *water, I was induced* to determine exactly the uniform and constant law, which connects these phenomena, and which, at the first glance, appear to be quite independent of each other. By means of a very simple apparatus, I can now produce instantaneously, and with economy, lumps of solid carbonic acid, weighing ½ to ⅔ oz. troy. These will probably be of some utility in experimental chemistry.

My early experiments on cold were made by directing a jet of liquid carbonic acid either upon the bulb of a thermometer, or upon tubes in which the substances intended to be exposed to the action of cold were enclosed. This mode has the disadvantages of wasting a large quantity of acid, and leaving some uncertainty as to the maximum of cold produced. The abundance and facility with which I can now obtain solid carbonic acid has furnished me with a mode of experimenting far more preferable.

The bulb of a thermometer being inserted in the centre of a small piece of solid carbonic acid, the index will, at the end of one or two minutes, become stationary, and mark −130° F. (= 162° below the freezing-point).

A few drops of ether or of alcohol, thrown upon the solidified mass, produce no appreciable modification, either more or less, in the temperature. The ether forms a mixture, partly fluid, and of the consistence of sodden snow; but the alcohol uniting with the solid carbonic acid, congeals, and produces hard and brilliant ice, of a demi-transparency. This congelation of anhydrous alcohol takes place in the state of union only; if the alcohol be preserved isolated in a silver tube, in the middle of a mass of solidified acid, it undergoes no change of form. The mixture of alcohol and solid carbonic acid begins to melt at −121° F. (= 153° below the freezing-point). Arrived at this degree, the temperature suffers no further change. So that at this extreme limit, there may be obtained a point as fixed as that of melting ice.

If, after having formed a little cup of solid carbonic acid, and about ⅓ oz. troy of mercury be poured into it, the metal will be seen to congeal in a few seconds, and to continue in its new form so long as there remains an atom of solid carbonic acid; that is to say, during twenty or thirty minutes, if the weight of the cup be nearly ⅓ oz. troy.

I have said that the addition of ether

or of alcohol does not increase the actual degree of cold; but by giving to the solid carbonic acid the property of wetting the bodies, and adhering more intimately to their surfaces, these substances greatly augment the frigorific effect. One volume of solid carbonic acid, upon which has been poured a few drops of ether or alcohol, can then congeal 15 to 20 times its weight of mercury.

The rapidity with which the solidification of the mercury takes place, the mass which may be acted upon, (which, with facility, may exceed half a pound,) and the permanency of the change of form, (which may be maintained as long as desired, by merely taking the precaution to place the *metallic button* on a bed of solid carbonic acid,) induce me to believe that this mode of solidifying mercury will henceforth be substituted for those which have been in use to the present time.—THILORIER.

Frozen Mercury.

M. DULONG exhibited lately to the *Académie des Sciences* a mass of mercury frozen by the process of M. Thilorier*, which weighed about 1¼lb. troy.

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* See p. 493.

Temperature of the Geysers.

THE temperatures of the Geysers (hot vapour-springs) in Iceland, have lately been taken with great care by M. Lottin. That of the Great Geyser, at about 80 feet deep, was found to be 257° Fahr., = 45° above the boiling-point. The Small Geyser, situated close by, at about 50 feet deep, gave 232° Fahr. Water, at the surface of the ground, in a small hole, was found at a temperature of 214°.—ROBERT. *Letter from Iceland, July*, 1836.

Patent-Law Grievance. No. X.

THE penalties inflicted on the inventive genius of Britain during the present year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £42,000 !

N.B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives*,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

NEW PATENTS. 1836.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

NOVEMBER *cont.*

263. WILLIAM SNEATH, Ison-Green, *Nott.*, Lace-maker; for improvements in producing embroidery, or ornaments on muslins, silks, and certain other fabrics. Nov. 28.—May 28.

264. ALEXANDER STOCKER, Bordesley Iron-works, and HENRY DOWNING, French Walls Iron-works, Birmingham, *Warw.*, Gents.; for improvements in manufacturing rivets, screw blanks, and other articles. Nov. 29.—May 29.

TOTAL, NOVEMBER...22.

DECEMBER.

265. DAVID NIMES CARVALHO, Fleet-st., *Lond.*, Bookseller; for improvements in propelling or moving vessels, and other floating bodies, on water, and carriages on land, which improvements are applicable to *windmills* and other purposes. Dec. 3.—June 3. *For. Comm.*

266. ROBERT ARMSTRONG, Stonehouse *Devon.*, Doctor of Medicine; for improvements in the water-pressure engine, rendering it more generally applicable for raising water and other substances, and as a motive power. Dec. 3.—June 3.

267. MOSES POOLE, the Patent-Office, Lincoln's-Inn, *Middx.*, Gent.; for machinery for a method of generating power, applicable to various useful purposes. Dec. 3.—June 3. *For. Comm.*

268. JAMES CORBETT, Richmond-place, Limerick, *Ireland*, Professor of Music; for improvements in producing harmonic sounds on the harp. Dec. 3.—June 3.

269. JACOB PERKINS, Fleet-st., *Lond.*, Engineer; for improvements in steam-engines, furnaces, and boilers, parts of which improvements are applicable to other purposes. Dec. 3.—June 3.

270. GEORGE SULLIVAN, Morley's Hotel. Charing-Cross, *Middx.*, Gent.; for improvements in machinery for measuring fluids. Dec. 3.—June 3. For. Comm.

271. Henry Booth, Liverpool, *Lanc.*, Esq.; for improvements in the construction and arrangement of railway-tunnels, to be worked by locomotive engines. Dec. 3.—June 3.

272. Thomas Don, James-st., Golden-sqr., *Middx.*, Gent.; for improvements in preparing and drying grain, seeds, or berries, and for manufacturing them into their several products, which improvements are applicable to other useful purposes. Dec. 3.—June 3.

273. William Bryant and Edward James, Plymouth, *Devon.*, Merchants and Co-partners, being of the people called Quakers; for improvements in the manufacture of liquid and paste blacking, by the introduction of india-rubber, oil, and other articles and things. Dec. 3.—Feb. 3.

274. William Hancock, Windsor-place, City-rd., *Middx.*, Gent.; for improvements in bookbinding. Dec. 7.—June 7.

275. Henry Adcock, Mount-pleasant, Liverpool, *Lanc.*, Civil-Engineer; for improvements in the raising of water from mines and other deep places. Dec. 9.—June 9.

276. Frederic Burt Zincke, Jun., Crawford-st., Marylebone, *Middx.*, Esq.; for the preparing or manufacturing of a leaf of a certain plant, so as to produce a fibrous substance not hitherto used in manufactures, and its application to various useful purposes. Dec. 9.—June 9.

277. Samuel Pratt, Peckham-rye, *Surry*, Gent.; for improvements in the construction of knapsacks, portmanteaus, bags, boxes, or cases for travellers. Dec. 9.—June 9.

278. Lemuel Wellman Wright, Manchester, *Lanc.*, Engineer; for improvements in machinery or apparatus for bleaching or cleansing linens, cottons, or other fabrics, goods, or other fibrous substances. Dec. 9.—June 9.

279. John Yates, Limehouse, *Middx.*, for improvements in tram-roads, or railways, and in the wheels or other parts of carriages to be worked thereon. Dec. 9.—June 9.

280. George, Marquis of Tweeddale; for an improved method of making tiles for draining soles, house-tiles, flat roofing-tiles, and bricks. Dec. 9.—Feb. 9.

281. John Melling, Liverpool, *Lanc.*, Engineer; for improvements in locomotive steam-engines, to be used upon railways or other roads, part or parts of which improvements are also applicable to stationary steam-engines, and to machinery in general. Dec. 15.—June 15.

282. Richard Thomas Beck, Little Stonham, *Suffolk*, Gent.; for an improved apparatus for obtaining power and motion, to be used as a mechanical agent generally, which he intends to denominate Rotæ Vivæ. Dec. 15.—June 15. *For. Comm.*

283. William Sharpe, Glasgow, *N. B.*, Merchant; for improvement in the treatment of cotton-wool, in preparation for manufacturing the same into yarn and thread. Dec. 15.—June 15. *For. Comm.*

284. Robert Walter Swinburn, South Shields, *Durham*, Agent; for improvements in the manufacture of plate-glass. Dec. 15.—June 15.

285. James Tarry Hester, Abingdon, *Berks*, Surgeon; for an improvement in the constructing of chairs. Dec. 15.—June 15.

286. Thomas Routledge and Elijah Galloway, Water-lane, *Lond.*, Gents.; for improvements in cabriolets and omnibusses. Dec. 19.—June 19.

287. Thomas Elliott Harrison, Whitburn, *Durham*, Engineer; for improvements in locomotive engines. Dec. 21.—June 21.

288. Andrew Smith, Princes-st., St. Martin's, Westminster, *Middx.*, Engineer; for improvements in the construction of standing rigging, and stays for ships and vessels, and in the method of fitting or using it, and in the construction of chains applicable to various purposes, and in machinery or apparatus for making or manufacturing such rigging and chains. Dec. 21.—June 21.

289. John Crighton, Manchester; for improvements in the construction of cylinders used in carding-engines, employed for carding cotton, wool, silk, and other fibrous materials. Dec. 21.—June 21.

290. James Potter, Manchester, *Lanc.*, Cotton-spinner; for improvements in spinning-machinery. Dec. 21.—June 21.

291. John Swindells, Manchester, *Lanc.*, Manufacturing Chemist; for improvements in the process of effecting the decomposition of muriate of soda or common salt. Dec. 21.—June 21.

292. George Houghton, High Holborn, *Middx.*, Glass Merchant; for improvements in the construction of lamps. Dec. 21.—June 21. *For. Comm.*

293. Stedman Gillett, Guildford-st., Gent., and John Chapman, Paddington, Merchant, *Middx.*; for improvements in that description of vehicles called cabs. Dec. 21.—June 21.

294. William Gossage, Stoke Prior, *Worc.*, Chemist; for improved apparatus for decomposing common salt, and for condensing and making use of the gaseous product of such decomposition, also certain improvements in the mode of conducting these processes. Dec. 24.—June 24.

295. Bennet Woodcroft, Mumps, *Lanc.*, Gent.; for an improved mode of printing certain colours on calicoes and other fabrics. Dec. 24.—June 24.

METEOROLOGICAL JOURNAL FOR NOVEMBER, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer.		Daily Temp.	Solar Var.	Rad.	Clouds.		Winds.		Direction of Wind		Lunation.	WEATHER, &c.
						Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Tues.	1	30·200	45°	30·103	46°	26°5	41°4	33°9	14°9	24°	7	10	0	1	SW	SW	☾	Complete thaw; small rain; Even. cloudy & close.
Wed.	2	30·050	49	29·983	52	40·0	53·5	46·7	13·5	37	10	9	1	1	W	W		High temperature; cloudy; great depos. of moisture.
Thurs.	3	29·925	54	29·758	56	41·2	51·4	46·3	10·2	37	10	6	0	2	WSW	WSW		Cloudy; 5 P.M. violent squall, hail, preced. by lightning.
Friday,	4	29·658	54	29·550	54	40·0	46·2	43·1	6·2	39	9	10	1	2	SW.W	SW		Overcast, scanty rain, generally gloomy. [& lightning.
Satur.	5	29·220	54	29·351	55	40·3	45·5	42·9	5·2	37	5	4	2	2	W.WNW	WNW		Rain at Night; *cumuli*, *cum.-str.*, &*nim.*; light rain, hail,
SUN.	6	29·550	50	29·542	52	31·8	43·9	37·8	12·1	29	2	3	1	2	wb S	W		Hoar frost; fine *cumuli*; 5 P.M. a dark *nimbus*, with
Mon.	7	29·725	49	29·809	50	30·0	43·0	36·5	13·0	29	1	3	1	2	W	W b N.		Much rain; fine loose *cumuli*. [rain&hail; clear Even.
Tues.	8	30·079	47	30·150	49	26·1	38·5	32·3	12·4	25	0	0	1	1	SW.W	S W		Ditto ditto; *cirro-cum.* A.M.; Afternoon cloudless.
Wed.	9	30·001	49	30·005	52	29·7	51·0	40·4	21·3	28	8	9	3	2	SW	SSW	●	*Cirro-cum.*; mild and windy; Night, wind and rain.
Thurs.	10	29·704	54	29·705	56	40·2	54·4	47·3	14·2	40	6	10	2	2	W	SW		*Nimbi* and *cumuli*, with showers.
Friday,	11	29·500	54	29·525	55	38·7	50·7	44·7	12·0	35	10	10	2	0	SE. S	SE		Rainy, damp; Evening slightly misty, calm.
Satur.	12	29·926	55	29·952	56	34·5	46·2	40·4	11·7	34	10	5	0	1	W SW	W		*Stratus*, dense fog till 9 A.M.; after. fine, mild; *cir.-str.*
SUN.	13	29·774	56	29·708	57	40·8	55·5	48·1	14·7	37	10	10	3	3	SSW	SSW		Rain & wind, overcast; *cirro-stratus* & *scud*. [at night.
Mon.	14	29·750	56	29·798	57	38·0	46·4	42·2	8·4	37	1	5	1	1	WSW	W		Fine, air drier; fine clear Evening.
Tues.	15	30·075	53	30·100	55	32·8	45·2	39·0	12·4	31	2	10	2	1	W	W	☽	Clear A.M.; Afternoon overcast; *cirro-stratus*.
Wed.	16	30·042	54	29·880	54	41·6	52·0	46·8	10·4	39	8	8	1	2	SW	SSW		Cloudy in general; Night, small rain with wind.
Thurs.	17	29·548	55	29·506	55	43·5	46·1	44·8	2·6	42	5	3	2	2	W	WSW.S		Fine *cirrus*; Evening, some light rain, windy.
Friday,	18	29·188	54	29·248	54	38·0	43·4	40·7	5·4	34	3	4	2	3	W	W		Fine A.M.; *cirri* in patches; clear Evening.
Satur.	19	29·350	50	29·258	51	32·0	43·0	37·5	11·0	30	0	10	1	3	W	W.NW		Hoar frost P.M., cloudy with showers; Evening windy,
SUN.	20	29·850	50	22·982	50	34·5	42·3	38·4	7·8	32	5	3	2	1	N	N		*Scud*, fair; Evening and Night clear. [and rain.
Mon.	21	30·200	49	30·149	49	30·1	35·8	32·9	5·7	29	10	10	0	1	W^LY	W^LY		Foggy, with *stratus*; cloudy at night, with scanty rain.
Tues.	22	30·035	49	29·960	49	32·5	39·5	35·7	7·0	32	10	10	0	1	E	E		*Stratus* and mist; Night, wind and rain.
Wed.	23	29·358	51	29·451	52	36·6	50·0	43·3	13·4	37	7	4	4	4	W	W	○	Wind very high; *scud* and loose *cumuli*.
Thurs.	24	29·558	51	29·600	52	35·5	42·5	39·0	7·0	32	1	5	2	2	wb N	W		Loose *cumuli* and *scud*; clear frosty Night.
Friday,	25	29·802	49	29,805	48	29·1	35·0	32·1	5·9	27	10	10	1	0	W	SE		*Cirro-cum.* from N.W.; dense *stratus*; rain at night.
Satur.	26	29·452	47	29·354	48	32·0	50·0	41·0	17·8	31	10	10	0	0		W SW		Misty and close, very damp, rain; warm Night with
SUN.	27	29·620	52	29·604	54	43·5	53·5	48·5	10·0	42	10	10	3	2	S.W.	SW		*Scud*; high wind at Night. [wind.
Mon.	28	29·346	58	29·358	58	48·6	56·8	52·7	8·2	48	8	10	4	4	SW bW	S W b W		[Evening, lightning.
Tues.	29	29·070	59	29·420	59	50·0	57·8	53·9	7·8	49	10	5	5	6.4	SW	WbNSW		Stormy; extremely violent between 11 A.M. & 2 P.M.;
Wed.	30	29·589	58	29·601	58	43·1	48·0	45·6	4·9	40	10	10	0	1	vari.	NW		Rainy and misty; *stratus*; a calm.
Mean		29·709	52	29·707	53	36·71	46·95	41.81	10·24									

Bar. Max. 30·200 in. on the 1st.
Bar. Min*. 28·860 in. 29th.

Ther. Max*. 57·8° on the 29th.
Ther. Min. 26·1° 8th.

Lowest point of Rad. 24°, on the 1st.
Rain fallen 3·175 in.

* Coincident with the great Storm.

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